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ADDRESS

ON

SOME NEW ASPECTS OF THE CHOLERA QUESTION SINCE THE DISCOVERY BY KOCH OF THE COMMA BACILLUS.

Delivered before the Medical Society of the District of Columbia, March 16, 1887.

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LATELY U. S. CHOLERA COMMISSIONER.

I appreciate the honor of an opportunity of coming before your learned body this evening to say something, although in a desultory manner, about some of the new aspects of the cholera question since the discovery of the comma bacillus of Koch, particularly those aspects which have more forcibly struck me in the study which I have had the opportunity to make of this question during my wanderings in Europe and India, under the direction of the President of the United States, for the purpose of investigating the cholera subject, with a view of making a report to the President concerning my labors. The subject is so vast that, of course, it would be utterly impossible to speak to any extent of all the features of cholera. In fact, I can only very briefly advert to some of the points which I think worthy of bringing to your attention to-night. It is unnecessary before this Society to go much into the history of the investigations of Koch and his associates of the German Commission, or into the investigations of other Commissions which have followed him in various fields concerning this matter. At the commencement of the invasion of Egypt, in 1883 by Asiatic cholera, it has been pretty well agreed, as you know, by most of the medical world, including the gentlemen, who, under the direction of the Marine Hospital branch of this Government, made an investigation and a most valuable report of the last cholera epidemic in this country—that of 1873—that the disease was in all probability due to a specific action of a specific virus, which in all probability was of material form, which had a natural habitat in some part of India, and was exotic in the rest of the world. They regarded cholera as a contagious or an infectious disease; that is transmittible from man to man and, therefore, transportable from place to place in various ways. So that, when Koch undertook to discover this specific germ he did nothing more than endeavor to bring before the eyes an object which already was believed to have an existence.

And, notwithstanding the fact that various micro-organisms in past years had been found associated with cholera, because of the fact that Koch was the recognized leading bacteriologist of the world—with the exception perhaps of Pasteur—and because of the fact that in all of his announcements to the medical world of his previous discoveries he had universally, as I understood without exception, received a wide confirmation of the announcements which he had made. Therefore more than a usual degree of expectancy was created by the mission of Koch to Egypt and India, with a view of undertaking an investigation of the etiology of cholera. When his announcements were made, in various bulletins to his Government, of the discovery of a peculiar organism in the dejecta of cholera patients, with characters enabling one to distinguish it readily from all other micro-organisms; and when he announced at the same time that this micro organism was not to be found in any other disease, and therefore constituted a reliable diagnostic sign indicative of Asiatic cholera, many medical men throughout the world were already predisposed to accept these announcements as at least worthy of serious investigation. The result of his discoveries confirmed the conviction of those who believed in the existence of a living contagium as the cause of cholera, regarded cholera as a contagious or communicable and a transportable disease, and also thought that in order to prevent visitations of the rest of the world by cholera, some quarantine measures and other means of prevention, as absolutely necessary to be enforced.

The English Government was being most interested in the question of quarantine, and held—especially their representatives in India—that there is nothing contagious about the disease, that it is no more transmittible from place to place than malaria, and that measures of quarantine, and other restrictions are not only useless, but unjustifiably harmful to the conduct of trade between the East and the West. That Government was, therefore, naturally the first to send out another Commission to the scene of Koch's labors, for the purpose of rëexamining his investigations. As you know, that Commission returned to London in due time, and published a report in which not only the conclusions of the German Commission were contested, but almost every detail of the report of that Commission was flatly contradicted; at the same time the report of the British contained indirect statements—I should rather say contained statements which indirectly supported the

assumption very widely entertained by high officers of the British Government, and particularly those in India, of the non-infectiousness of cholera dejecta. Following this contradiction many others occurred in due course, but is not necessary for me here, nor have I the time to relate them.

The United States Government, being as much interested in the question of the etiology and the prevention of cholera as any other by reason at least of its sixty millions of inhabitants (a number almost twice as great as the inhabitants of any European country outside of Russia), concluded that it would be advisable to have the grounds of these differences of opinion investigated by one of their own representatives.

I went to Europe in the fall of 1885, when cholera was almost ended there. It was, however, still existing in the City of Palermo, and in some parts of Spain. I went to Palermo because that was the largest field of investigation at the time, and I remained there until the epidemic ceased. From there I went to Spain by way of the Italian and French Riviera, stopping at different places which had been visited by cholera in 1884 and 1885, for the purpose of examining into their hygienic condition, and of learning something of the history of the epidemic in them. I reached Spain when cholera was no longer existing in that country—that is, acknowledged officially as existing—except in the south, along the Straits of Gibraltar. I had ample opportunity to make bacteriological investigations concerning the relation of the comma bacillus of Koch to Asiatic cholera, as well as the relation of other germs to that same disease, as also opportunities to inform myself concerning the grounds of difference between Koch and his opponents as regards matters of fact. These opportunities also were somewhat extended by my visit to India. I had no difficulty whatever in confirming the statements of Koch with regard to the constant existence of the comma bacillus, in certain stages of Asiatic cholera in the dejecta of patients suffering from that disease; and in confirming also his statements as to the distinctive characteristics of the bacilli distinctions, which enable one to separate them readily from all other micro-organisms known up to the present time.

As you know, the comma bacillus of Koch is a curved organism, and you know also that other curved bacilli have been found in various localities and in various diseases. The statements of Dr. Lewis, of England, were among the first announced against even the assumption of Koch for the diagnostic value of his comma bacillus. He found a curved bacillus very much resembling, under the microscope, the comma bacillus of Koch in the normal secretions of the human mouth. Curved bacilli were also found in the intestinal contents of diarrhœa and dysentery patients, in leucorrhœal discharges, in ordinary river water, in well water, in sewage water, and what not. I myself, before I had paid much attention to the bacteriological study of the question of cholera, had met with them in the exudation of the lungs of pneumonia patients, had seen them in diarrhœa, in leucorrhœal discharges, and also in river water. I found even a curved bacillus once in some broken-down beef broth which had been exposed to the air of my laboratory.

The peculiar characteristics of the gelatine tube and plate cultures of the comma bacillus and of some of the other curved bacilli, such as the Finkler and Prior bacilli, which were found by these authors in the dejecta of cholera nostras, and the curved bacillus found by Deneke in decomposed cheese are fairly shown both in the photography and the camera-leucida drawings. These appearances in the plate and tube cultures, as you see, enable one to easily distinguish between the various growths.

I can only say that I agree entirely with Koch as to the diagnostic value of the comma bacillus, and therefore disagree with the conclusions published in the first report of the English Commission, with Dr. E. Klein at its head. I would say at this point that I regard this as the most important point of Koch's investigations. In the discovery of a reliable and an easily applicable distinctive means of diagnosis of Asiatic cholera at the very first moment of invasion of a locality or a country, I consider that Koch has conferred upon the human race an inestimable benefit for, with the proper application of this means of diagnosis, health authorities and Governments will now more than ever before have it in their power to prevent an invasion of cholera, and cut short the spread of an epidemic. The value of this discovery, as I have said before, I consider incalculable.

But, as to the other part of the claim of Koch—for his claim is two-fold—in the first place the discovery of a reliable diagnostic sign, in the second place, the discovery of the active agent or cause of Asiatic cholera—I think there is still reasonable ground for doubt. The fact that, so far as we know at present, there are none of the domestic animals which are naturally susceptible to this disease makes it difficult to settle the question as to the pathogenic power of the comma bacillus by means of experimentation upon animals. It is true, that a *quasi* cholera has been and can be readily produced in Guinea-pigs, and according to some also in dogs and rats, by the inoculation of these animals with pure cultures of the comma bacillus of Koch; but there is lacking the complete chain of symptoms with which we are familiar in genuine Asiatic cholera in man. The vomiting is usually absent, and also the diarrhœa; nevertheless, a cyanosis, or algidity, and sometimes the cramps, and the death of the animal with the well known characteristic intestinal contents are met with after the inoculations. I myself have performed numbers of these inoculations and satisfied myself as to that point. But still there is a question—at least in the minds of a great many—as to whether the symptoms following inoculations of these germs are really those of Asiatic cholera or not, and on that account it seems to me that the question of pathogenesis of cholera must for a time remain open. In the course of my remarks I shall have occasion to say something about the results of the human inoculations which were extensively performed in Spain, with cultures of the comma bacillus.

I will remark here, that I found in my visit to India, a very good explanation of some of the differences between the English and the German Commissions as to the diagnostic value of the comma bacillus. Koch distinctly states in all of his communications (and every one who has had any experience in the investigation of this matter knows it to be true) that by means of morphology alone it is absolutely impossible to distinguish between the comma bacillus and various other curved organisms. As you will see by some of those photographs and drawings which have been handed round, there is no great difference as to shape and the size between the comma bacillus of Koch and the bacillus of cholera nostras, or that of Finkler and Prior, and the cheese bacillus discovered by Deneke; and I may say the same of the new curved bacillus of which I spoke, or of those found in the normal mouth, in river water and elsewhere. No man more positively states this than Koch himself, even in his very first communications about the matter. He most distinctly stated that it is absolutely necessary for the recognition of the comma bacillus, and the separation of it from other micro-organisms, that plate cultures and tube cultures in flesh-peptone-gelatine be resorted to. Without this means it is utterly impossible for anyone, even Koch himself, to distinguish between these various curved bacilli. The great fault, as we consider it, in the investigations of the English Commission in India, as refers to this matter, is that they did not seem to have fully appreciated the necessity of resorting to gelatine cultures for the purpose of distinguishing the micro-organisms which they were examining; in fact, in Calcutta where most of their observations were made, as far as I know they did not resort to any sort of culture of these micro organisms, but relied entirely upon the microscopic examination of simple cover-glass preparations. After leaving Calcutta, and on their way homeward stopping at Bombay, it appears that they did resort to some cultures in Agar-Agar. But Koch had already explained, and every one who has since that time practically worked in the matter, knows that there is no characteristic of the colonies of the comma bacillus in Agar-Agar plate cultures which enables them to be safely distinguished from the other curved bacilli. So that the method of examination of these micro-organisms followed by the English Commission was virtually inadequate to distinguish between them and the others. They erred, to my mind, in assuming that every curved bacillus which they found in the intestines of cholera patients or in other diseases, or in tank water and other places, were to be regarded as really the comma bacilli of Koch. On their return to England, these faults were pointed out by one of the most distinguished bacteriologists of that country, Mr. Watson-Cheyne.

The head of the English Commission has recently published a communication, although not of the same official character as his official report to the British Government.

While Dr. Klein still adheres to his denial that the comma bacillus has any causal relation to Asiatic cholera, he admits now, as I understand, the mistake which he had previously made in stating that in gelatine plate and tube cultures the comma bacillus had no characteristic which would enable it to be distinguished from ordinary germs of decomposition. That statement he has withdrawn; and he has practically admitted the diagnostic value of the comma bacillus of Koch for the purpose of recognizing Asiatic cholera and distinguishing it from cholera nostras, malignant malaria, and other diseases.

I now drop the subject of bacteriology and come to another point which had been more or less overlooked in an extraordinary way up to the time of the present epidemic, although scattered observations appear here and there in the history of the subject—observations, I may say, however, made by the most distinguished writers on the subject. I refer to certain peculiarities which have been noticed in the course of cholera in individuals, in villages and in countries. James L. Bryden was the first to point out the periodicity of epidemics of cholera in India, but did not undertake to explain it except by the influence of the monsoon. Subsequent investigators of cholera in India have also noted this periodicity. The periods of recurrence of cholera epidemics in different parts of India range from three to four years as a rule, whilst the periods of recurrence of small-pox vary from five to six years. For the provinces outside of the endemic area, there is a very marked periodicity and a very considerable similarity between the rise and fall of cholera and of small-pox. Charts showing the annual mortality from cholera in the other provinces strikingly indicate the periodical character of epidemics of cholera in various sections of India.

This periodicity has attracted the attention of many investigators, although the fact seems to have passed by without any general notice, and yet it seems to be closely connected with the explanation of some of the important peculiarities of cholera in India as well as elsewhere. It has been noted in India, for instance, that instead of cholera advancing down the course of the rivers it invariably travels up them in certain countries. In the Eastern part of India in the epidemic years cholera travels up the Ganges toward the northwest, instead of moving down stream. The movement of an epidemic down the Ganges to Bengal is unknown in the history of cholera in India; the only part of Hindostan where it often goes down with the current is in western and southern India.

Another remarkable and hitherto unexplained fact is that cholera dies out in the localities visited without having smitten the whole population; and the further fact that where large percentages of the inhabitants of a place invaded by cholera flee to a distance and subsequently return to their homes before the epidemic has entirely ceased, the ravages of the epidemic continue mainly among the returned refugees; in other words, among the people who have not become subjected and habituated to the cholera influences which are still prevalent in the town to which they return is still more inexplicable. Then again there is the strange fact that a recurrent

attack of cholera is an extremely rare thing. It is indeed so rare that very few people whom I met with in the countries where they severely suffered from cholera, had ever seen a recurrent attack of it in the same epidemic. Recurrent attacks in a second epidemic are not extremely infrequent, but it appears to be quite as rare to observe a recurrent attack of cholera during the same epidemic in a person who has recovered complete health after his first attack, as to see such a thing in scarlet fever or measles, or I might say also, in small-pox.

In speaking of these questions I might refer to the researches of my friend, Dr. John L. Keating, of Philadelphia, who is greatly interested in the diseases of childhood, and who, at my request, kindly looked up some records regarding early recurrences of scarlet fever and other infectious diseases for use in my official report. I regret that time does not permit me to lay before you the valuable table which he has furnished, but I will read you a portion of his letter accompanying it, as follows: "I may say that as a rule all the zymotic diseases protect from recurrent attacks, or at least have a tendency to lessen the severity of these recurrences, the exceptions which prove the rule occurring very frequently in each disease." "In my table I have not included the relapses which are most frequent in typhoid fever, but I have only mentioned those attacks that have been followed by restoration to health, and in which after a shorter or longer period of time there has been a recurrence of the disease; measles present these peculiarities oftener than any of the others, but it is by no means uncommon in variola, scarlet fever and whooping cough." In his table, in the main relating to well authenticated early recurrence of scarlatina, many instances of recurrence in two or three weeks of attacks of scarlet fever and measles are recorded.

When in Spain I addressed a series of twenty-four questions bearing upon the cholera in that country to the physicians of every village in that country which had suffered from cholera in 1885. Among those twenty-four questions was one which requested the physician to inform me whether he had any personal knowledge of a recurrent attack during the same epidemic; if so, to give me the details. Among the large number of replies to the 2,500 letters which I thus addressed, I received only eight which related to recurrent attacks. Six of those eight recorded, according to the details given in the letter, appear to have been *relapses* before the establishment of perfect health, after imprudences in diet, and imprudences of other kinds, rather than recurrent attacks of cholera, and the number was thus reduced to two, which may have been genuine recurrences or have been attacks of cholera morbus (or nostras).

The French Academy likewise addressed a series of questions to the villages in France which had suffered from cholera in 1884, and one of the questions in their circular letter bore upon the same point. In the replies received an account of ten recurrences (or relapses) the distinction between relapse and recurrence was not made in their replies.

These are matters which have attracted the attention of some of the most distinguished observers, and caused them to admit an immunity following an attack of cholera, as you will notice when I cite some of the history of the opinions of authorities upon this point. Among the early writers is Moreau de Jonnes (1832) who said that: "If a few rare or doubtful cases are excepted, it (cholera) does not attack the same individual twice, although he finds himself under the same condition as when he contracted the disease." Dr. Sámano, of Spain, in 1858, declared that "genuine cases of relapses are few, and recurrences are still more rare," and Lebert, one of the most distinguished investigators of his time, firmly believed in the immunity which an attack of cholera confers.

Many of the International Health Congresses discussed this matter, particularly that of Constantinople in 1866. They distinctly admitted in their conclusions, adopted by a unanimous vote, that there is a certain amount of immunity which is produced in a town by a visitation of cholera. Speaking of the pernicious effects of crowding together of people, they pointed out as an amelioration that an habituation to the choleraic influence and the experiencing of choleraic attacks counterbalance these bad effects; and they said the same of crowds upon ships. The next in order of time (1868) is Griesinger, who distinctly stated that next to good hygiene and pure water, an attack of cholera is the greatest protection against the invasion. Colvin, (in 1879) although he distinctly expressed as his conviction that there is no such thing as immunity against an attack of cholera, in the course of his book refers to facts which most positively support the assumption of an immunity, particularly in the history he gives of cholera in the Crimea during the war, and in the accounts of some military epidemics.

In later times, 1883, Dr. Proust, the distinguished hygienist of the University of Paris, as also Fauvel, another distinguished hygienist of France, after a thorough discussion of the facts of the subject, most positively declare that there is an immunity following an attack of cholera, more or less lasting, as there is with the other infectious contagious diseases, and they class cholera in the latter category. Dr. Proust ends his remarks on that subject thus:

"Indeed the facts newly acquired by science relate to questions of immunity and illuminate them from a side heretofore unknown.

"The etiology and prophylaxsis of cholera in particular may deduce from it (immunity) new indications.

"These facts moreover, appear to be the expression of a law which embraces the whole of another special category of pestilential diseases due to a contagium and leaving after them an immunity more or less lasting."

Griesinger, Wunderlich and Pettenkofer, in their "Cholera Regulation," most unequivocally affirmed the existence of an immunity of considerable duration and force. And lastly, we come to Koch, who in the discussions of the cholera question before the recent Berlin Cholera Conference, after relating the facts upon which his conclusions were based, most positively expressed his acceptance of an immunity, more

or less lasting, from an attack of cholera, and classified that disease among other zymotic diseases which are followed by immunity. He affirmed that unless the immunity theory be accepted, there is no possible way of explaining why it is that epidemics cease in a town without attacking the whole of the people who are living together under miserable conditions, and why it is that recurrences are so extremely rare, and there is no other way of explaining satisfactorily the peculiarities of the course of cholera in India, viz., the periodicity of the disease there, and the fact that it travels up rivers in the East instead of going down them. But I cannot occupy your time further in the citation of distinguished investigators who up to the present time have regarded cholera as among those diseases which confer a more or less lasting immunity after an attack, and I shall be obliged to leave unsaid much of that which I intended to speak.

I have spoken of the immunity which follows an attack of cholera. I believe immunity to be as certain a sequel of cholera as it is of most of the other zymotic diseases. Otherwise it is impossible to explain why it is that a person in the early part of a long epidemic suffers the disease and recovers, with his digestive apparatus in a miserable condition, perhaps with chronic dyspepsia and chronic diarrhœa for weeks and even months, and yet that person, living under the same conditions under which he took the disease, passes through the epidemic free of a second attack, especially so in view of the fact, as is well known, that in a person who is not already protected by a previous attack, digestive disturbances are very sure to dangerously increase the predisposition to an attack of cholera. And yet, as I have pointed out, it is the rarest thing to meet with a second attack of cholera in the same person in the same epidemic.

This brings me to the question of the Ferran anticholeraic inoculations. The German Commission stopped with the announcement of their belief in the immunity which follows cholera, not dreaming of making any practical applications, notwithstanding their reference to the practical use of this immunity which the Indian Government makes in their directions for the government of the troops during epidemics of cholera. I allude to the standing orders to select the nurses from the native population, and especially from that which has recently experienced an epidemic of the disease. Ferran, as you know, became notorious throughout the world from his announcement of a preventive inoculation against cholera. Previous to this announcement, I may say, that Dr. Ferran was regarded in Spain as one of the leading, if not the most prominent bacteriologist in the country, he having been awarded a year or two before a gold medal by one of the medical institutions in Madrid, for researches relating to bacteriology. His reputation as a bacteriologist and as an earnest, reliable investigator was such, that when the cholera broke out at Toulon, he was selected by the Government of Barcelona as the head of an official Commission to go to Toulon for the purpose of investigating the cholera, and reporting officially. His first communication on the subject of inoculation was addressed to the municipality of Barcelona, and some time after his return from Marseilles and was referred to the Royal Academy of Medicine, of Barcelona, which is regarded in Spain as perhaps the most distinguished medical body in that Kingdom, for their judgment of the report. That medical body appointed a Commission to investigate the grounds of Ferran's claim. The investigation ran through two or three months, comprising experiments by Ferran performed before them upon Guinea-pigs, the making of cultures, and the performance also of human inoculations, to a certain extent, among the members of that Academy mainly. The report of that Academy was confirmatory in most respects of the announcement of Ferran, and it was this report upon the Ferran question which first reached and excited the interest of the rest of the world.

The French Government, among the first, as also the Belgian, sent a Commission to Spain to investigate the grounds of Ferran's claim. Upon their arrival in Spain they laid down a programme for Dr. Ferran, but he refused to follow it, suggesting, on the other hand, one of his own for them to follow. The result, as you know, was that the official investigation of the Commissioners ended before it began, and the Commissioners went back to their respective countries, much chagrined, and reported unfavorably Ferran. Still, the medical profession and the public of France were not quite satisfied with this report, and another Commission was sent, with Dr. Brouardel at its head associated with two other physicians, one of them a recently graduated medical student, who was selected mainly on account of his knowledge of the Spanish language. (Neither Dr. Brouardel nor the other Commissioners were able to speak Spanish, and none of them, by the way, were bacteriologists, however distinguished they are in other respects.)

The second Commission met with pretty much the same reception as had the first, and undertook to enforce the same sort of programme which the first had attempted to lay down for Ferran, and with no better results. After breaking off negotiations with Ferran, the second French Commission undertook to collect the statistics then existing (they were in Valencia in the summer) of the results of the human inoculations which were being so extensively practised by Dr. Ferran. In the meantime, however, the Spanish Government itself had appointed three Commissioners to investigate the Ferran question, and those Commissioners had reported that the inoculations were harmless, and that they were made with pure cultures of the comma bacillus of Koch, thus exploding many widely published statements to the contrary, and recommended that they be continued. The statistics collected for and presented to the Minister of Commerce of France by the second French Commission, and the criticisms which have been made concerning them, are practically the only quasi-reliable data which the rest of the world had of the results of the inoculations in Spain. The objections raised against the statistics obtained by Dr. Brouardel (such as they were, strongly in favor of the claims of Fer-

ran), were that they were not detailed enough; that they did not have any real official character, and that they were collected by the partisans of Ferran. The judgment of the world (myself among the number) of course naturally was most uncomplimentary to Dr. Ferran and to his anti-choleraic inoculations. When I reached Spain, however, I found that by that time the official statistics had been collected, and that they had been collected not by the partisans of Ferran, but by the representatives of the Spanish Government, who from the known history of the Ferran question in Spain, at least, cannot be accused of being friends of Ferran.

The Provincial Governments are somewhat peculiar in Spain in that these Governments change with the changes of the Cabinet at Madrid; so that the Provincial Governors always reflect the sentiments of their respective ministers at Madrid. It is no secret in Spain, but very well-known that the Ferran inoculations received no favor, and in fact, active hostility from the Minister of the Interior at Madrid, and the same sentiments towards Ferran personally, and towards his work, were reflected among the political officials throughout the various provinces in Spain, wherein the inoculations were practised. The same also may be said with regard to the Governments of the municipalities; they were also subject to the political influences of the Provincial Governments, and therefore also to those of the Government at Madrid. The Spanish Government ordered at the time of the appointment of the second Commission, that official and careful statistics should be collected concerning the Ferran inoculations, by the usual or by special officers for that purpose. In this manner provision was made for the collection of official statistics, which, by the way, had been collected and published some weeks and months after the publication of the unofficial statistics obtained by the Brouardel Commission. The official statistics are signed and attested in the first place by the physicians of the villages where the inoculations occurred; in the second place, by the municipal judge; in the third place, often also by the president judge of the judicial district in which the village is located; in the fourth place, they are attested by the Curé of the parish; in the fifth place, by the secretary of the municipality, and by the mayor; and lastly, they are attested by the notary public of the district. So that, of official formality, there seems to be no lack.

These official statistics, whatever value they may otherwise have, cannot now be subjected to the same charge which Brouardel made, very justly, against those which he obtained. They still lack detail in many respects, usually failing to indicate the social position and the hygienic surroundings of the persons attacked or inoculated. Such as they are, however, they seem very strongly to support the claim of Ferran that there is an immunity against cholera, produced by the inoculations of the comma bacillus of Koch. The nature of these statistics is such, and their results so striking, notwithstanding their great faults, that it now seems to me imperative that Governments of Europe and of this country, and serious scientists throughout the world should re-examine this whole matter for the purpose of getting at the real truth of the question as to whether there is or is not an immunity produced by the inoculations of cultures of the comma bacillus of Koch.

I referred a few moments ago to these inoculations, being the only ones which were made with the comma bacillus to any extent in the human subject. Ferran himself, and one or two other investigators, as a basis for attempts at inoculation in man, claim to have produced immunity in the inoculated guinea-pig. I may say just at this point, that I was very much surprised in India in an interview with D. D. Cunningham, the celebrated cholera authority there (known throughout the world for his investigations), who told me most positively that he had produced such a condition in guinea-pigs which he had inoculated hypodermatically with cultures of the comma bacillus of a certain amount, that it was impossible to kill the same pigs with subsequent inoculations which in fresh guinea-pigs would prove certainly fatal. This is, perhaps, the most important confirmation which the statement of Ferran concerning an immunity produced in animals by means of inoculations by the comma bacillus of Koch, has yet received. I mention this point for what value it may have. I refer to it also as an interesting matter, because it shows that Dr. Cunningham who, after the departure of Koch from India, set himself to work seriously to investigate the relations of the comma bacillus to cholera has radically changed his views concerning Koch's discovery since prosecuting those investigations. From an opponent he has come to be a strenuous advocate of the first part of the claim of the German Commission; that is, that with the comma bacillus we have found a reliable means of diagnosis of Asiatic cholera.

Now I will say, after this short digression, a few words about the result of the anti-choleraic inoculations in Spain, and then will close, feeling that I have already held your time too long.

The published official statistics comprise reports from twenty-two villages in Spain where the inoculations were performed, having among their inhabitants 30,491 inoculated, leaving 104,561 not inoculated. Among the inoculated there were 387 attacks, or 12.69 per thousand, and 104 deaths, or 3.41 per thousand, giving a mortality of the attacked of 26.87 per cent.; whilst among the non-inoculated there were 8,406, or 80.28 per thousand, and 3,512 deaths, or 33.50 per thousand, giving a mortality of the attacked of 41.80 per cent. Now these figures are striking, as you see. I have drawn a diagram which represents, according to the analysis of these statistics, the relative protection of the people against attacks and death by cholera insured by ordinary hygienic measures, sanitary cordons, disinfection, and other measures in vogue at the time, as compared with that of the inoculations. A short line drawn on the diagram being the unit of protection of the inhabitants in the inoculated villages against attack by cholera produced by the hygienic and other measures, this long line represents the degree of protection which has followed

inoculation: that is, a degree of protection 6.06 times greater than the degree of protection from attacks afforded by hygienic and other measures. This short line again being the unit which represents the protection from death by cholera of the general inhabitants afforded by hygienic and other measures, a longer line represents the amount of protection which, according to the statistics, seems to be afforded the inhabitants from death by cholera by the inoculations: in this case it is 9 84 times greater than the protection afforded by the usual measures of hygiene, disinfection, etc. This chart is based, I should state, solely upon the statistics furnished by one inoculation. Ferran himself claims (although the statistics are not full with regard to this point) that the height of the protection comes after a reinoculation; that one inoculation only partially protects, while a second inoculation completes whatever degree of protection there may be from the inoculation. For instance, it is a rare thing to find a death recorded among those reinoculated. Furthermore, I have not attempted to estimate the deaths among the inoculated who might have had the germs of cholera acting within them at the time of the inoculation.

I have diagrams representing the course of cholera in two villages where the inoculations were performed. In the village of Livia, where of a population of 9,460 there were 1,419 inoculated, leaving non-inoculated 8,041, among the inoculated there were 40 attacks and 7 deaths; among the non inoculated there were 561 attacks, and 354 deaths. In the village of Benifayó there is a population of 3,615, and among that number there were 2,723 inoculated, leaving 160 dead and 732 not inoculated; there were also 2,000 reinoculated. A great number of inoculations (2,315) were performed at this point in the course of the epidemic, a day or two before this sudden fall in the epidemic which you notice, and you will see that its course is suddenly brought to a termination.

Now I have made an analysis of the official Government statistics lately published by the Spanish Government of 361 villages in the province of Valencia, and in a few other provinces that were invaded by cholera, with a view of getting at the average course of the epidemic of 1885 in Spain. There were 361 villages, with a population of 996,546 inhabitants, the attacks by cholera being 88 per thousand of the inhabitants. The average duration of the epidemic in these 361 villages was forty-four days; this period was divided into a rise of fourteen days, a stationary period of eleven days, and a decline of nineteen days, the decline being longer than the rise. The epidemic in Spain in this respect coincided with the course of epidemics the world over.

I will end this Ferran matter by saying that, while I do not regard these official statistics of the results of inoculation as by any means conclusive, yet I consider them sufficient not only to warrant, but to imperatively demand that this question shall be thoroughly and scientifically investigated, a thing which has not yet been done, notwithstanding the visit to Valencia of numerous Commissions which have gone there for the purpose of investigating the matter.

When in Spain I investigated 361 villages where there has been an attack of cholera, and the following are some of the points which were brought out by that investigation: In the 361 villages cholera was introduced 211 times by persons already affected or bearing the germs of the disease within them; eighteen times by clothing; fourteen times by sick persons and soiled clothing. It was stated only once that merchandise was the means of the introduction of the epidemic, and only fifteen times did they attribute the introduction of cholera to drinking-water.

I am sorry that I have not time to speak of the hygienic condition of the people of Spain, and point out the fact that it is almost impossible for people living in the irrigated districts of Spain—which are, by the way, the portions which have always most severely suffered from cholera epidemics—to have water which is not contaminated either by their own sewage or by the sewage of villages immediately above them in the course of the irrigation canals; for these canals are used frequently not only as public sewers, when they pass through the town, but they are used also as public lavatories, and as a supply of water for drinking and other domestic purposes. And yet, in Spain and in Italy also, in studying the course of cholera, this matter is usually overlooked. In only fifteen villages did I find any reference to the possibility of introducing cholera by means of the irrigation canals in the replies sent to my inquiries.

One of the statements made by the foreign official Commissions that went to Spain reflected very severely upon the knowledge of Dr. Ferran of the rudimentary principles of histology, bacteriology and microscopy. I can only say that my personal experience in Dr. Ferran's laboratory was entirely different from that. I have photographs of some of his preparations, and I venture to say that no handsomer photographs have been made with a microscope anywhere in the world, and no more handsomely stained preparations of the comma bacillus, and no more pure specimens of the comma bacillus in gelatine have I seen in the laboratories of Berlin or of Paris than I saw in Dr. Ferran's laboratory, modest though it was, in the city of Tortosa, where he lives. I may say that, although Dr. Ferran may be mistaken, and probably is, in some of his claims, especially with regard to the morphology of the comma bacillus, and he may also be mistaken in his views about inoculation, my own personal experience with him, and what I could learn from disinterested sources in Spain from all sides, leads me to believe that instead of being the charlatan he has been represented, he is a gentleman, in the first place, and in the second place a well informed physician, an investigator, and a microscopist of no mean skill; and that whatever he may have stated about this matter of anti-choleraic inoculation, it is his honest belief.

I must apologize to you, Mr. President, and to your Society, for having so outrageously taxed your patience, and also for the disconnected manner in which I have had to bring some of these important points to your notice; but it has been impossible for me to do otherwise, inasmuch as during my stay for the last three or four weeks in Washington I have been constantly engaged in the completion of my official re-

port, which I hope to be able to place in the hands of his Excellency the President soon, and I have really not had the opportunity even to properly arrange in my mind what I should endeavor to say upon this subject.

ORIGINAL ARTICLES.

PERSISTENT VOMITING DURING LABOR RELIEVED BY ANÆSTHESIA.

Read before the Medical Society of the District of Columbia, March 21, 1887.

BY SAMUEL C. BUSEY, M.D.,

OF WASHINGTON, D. C.

Nausea and vomiting at the beginning or during the progress of labor is not uncommon. When occurring during the early period it usually ceases with the evacuation of the stomach, or only recurs occasionally. In some cases the patient will vomit at long intervals until the delivery is accomplished. Such vomiting is usually regarded by the laity as beneficial, the popular belief being that "sick labors are easy labors." Obstetricians do not seem to have attached any special significance to the occurrence of such gastric disturbances during the first stage of labor; but when persistent during the second stage, producing exhaustion and lessening the activity of uterine contractions, the safety of the patient may demand immediate delivery.

The complication of labor with persistent vomiting must be very rare, as I have not found a single case reported in detail, after an extensive examination of obstetric literature. Only a few authors casually refer to the subject. The following case, though probably not a unique observation, may prove instructive, in so far as it may attract attention to the subject, and, possibly, suggest a method of treatment not previously employed.

Previous History.—The previous history of this patient is instructive and important, not only because it may offer an explanation of the persistent vomiting, but because it will introduce the details of another rare complication which fortunately did not present the obstacle to delivery anticipated. The mother of the patient was the third child of an "old Englishman and a young Irishwoman," and was the only one of a large family of children afflicted with exostoses, which were mostly located on the long bones in proximity to the joints. The patient is one of eight children of this mother, all of whom inherited this peculiarity of the osseous system. In most of these children the exostoses are located in the neighborhood of the ankle and wrist joints. They have not been observed previous to the seventh or eighth year of age, and have been believed to have followed injury to the bone or joint involved.

The patient had a miscarriage at three and a half months, November 21, 1885, while residing in one of the Territories. Her statement is that she suffered during the continuance of this pregnancy, almost constantly, from nausea and vomiting; that the pains during the miscarriage were very severe, and that the placenta was not delivered until the fourth day. For several weeks following she was confined to her bed with fever, and for three months afterwards she "had hæmorrhages from the womb," so that she was greatly emaciated and reduced in health and strength. During her illness the nurse discovered an exostosis arising from the right ramus of the pubis, midway between the symphysis and inferior spinous process of the right ilium, nearly globular in form and measuring one and a half inches vertically. The patient had been aware of the presence of this tumor from early childhood, but as it had not perceptibly increased in size, nor occasioned any discomfort, had supposed it was normal until informed by the nurse to the contrary. The nurse had also volunteered the information that, in consequence of its position, she could not give birth to a living child at term.

In April, 1886, she consulted me, at my office, with special reference to this statement of the nurse, at the same time expressing a great desire to have a child, and a willingness to incur any reasonable risk. I was quite willing to assure her that the exostosis, located as before described, would not obstruct delivery at term, but declined to express the opinion that she could give birth to a living child, at term, *per vias naturales*, unless I could have the opportunity of a careful examination of the pelvic cavity. To this both the wife and husband finally consented and, by appointment, this exploration was made two days afterwards, at their residence in this city. This examination disclosed the existence of a sharply pointed exostosis jutting obliquely upwards from the inner surface of the ramus of the pubis immediately below the external and larger tumor, measuring (approximately) three-fourths of an inch in length. The upper border of its base seemed to be on the *linea ilio pectinea*; otherwise the pelvis seemed to be normal.

When informed of the existence of this exostosis the emphatic and definite statement was made, in the presence of her husband, that she was unwilling to continue, as she had done since her miscarriage, to deny to her husband the privileges and pleasures of conjugal rights, and that her desire to have a living child was so great that she was willing to incur any danger, short of certain death. These declarations were followed by the inquiry if the bony tumors could be removed, and if such an operation would be more or less dangerous to her than the birth of a living child? To this I replied that such an operation might be performed, but that I thought the chances of survival would be less than from some one of several obstetric procedures which might be expedient in the event that she should again become pregnant.

Then followed the graver question: Could she be delivered of a living child at full term? My reply was as follows: Premature delivery at the seventh, eighth, or eighth and a half month might, under certain circumstances to be ascertained during the progress of pregnancy, be considered the wisest procedure. If permitted to go to full term Cæsarean section or one of its modifications, or gastro-elytrotomy, might be considered the proper methods.

Either of these operations might secure the birth of a living child, but would be attended with considerable danger to her. That craniotomy might be less hazardous to her than either of the other surgical procedures referred to, but the child would necessarily be destroyed. She promptly and positively rejected craniotomy, and expressed a decided preference for one of the surgical methods because they seemed to her to offer better chances for a living child. I then added that I believed, under certain favorable circumstances, she might give birth to a living child at full term *per vias naturales*. These were: a female child, a small and flexible head, a vertex presentation either in L. O. A. or R. O. P. position, or, if the presentation and position were recognized in time, the change of either the R. O. A. or L. O. P. to one or the other of those positions of the vertex, so that the head would descend with the side and not the face or occiput towards the exostosis. If the presentation should be breech the chances would be less, but not necessarily fatal to the child.

She accepted these possibilities with the greatest pleasure, and with exultation expressed her determination to take the chances with the risks. We parted with the agreement that if she should become pregnant I would receive notice as soon as she was entirely satisfied of the fact. To this I reluctantly consented, for I would have preferred to have terminated my services with the expression of my opinion and been superseded by some colleague more anxious than I to witness the possibilities of the future.

Her womanly courage proved equal to her heroic enthusiasm. Late in July I received a note from her communicating the information that she was pregnant and requesting me to call. She dated her pregnancy from the 7th of May. I found her in perfect and robust health, full of enthusiasm, and equally determined as when we parted in April. The whole subject was again calmly and deliberately discussed. I left her with the injunction to give me immediate notice of any symptom of ill-health, and especially of the first movements of the fœtus. On September 8 I was informed that the first sensation of motion of the fœtus had been felt the day before. My examination of the pelvis verified the condition ascertained in April. I could not discover any other obstacle to labor than the exostosis before described. The date of confinement was then fixed for February 7, 1887. I left her with the statement that I would not, if then, interfere in any manner before the completion of the seventh month, unless something unexpected should occur. At this visit, as at every visit made by appointment during her pregnancy, she had a severe rigor with marked trembling. Similar attacks were not infrequent, and were attributed by the patient to "nervousness." They were always relieved by a potation of whiskey without any subsequent ill effects. These tremblings, usually lasting a half-hour, were so general and tumultuous that an examination could not be conducted until they had subsided.

After a careful examination in December, during the early days of the eighth month, I concluded to defer the induction of premature labor, because I was convinced the fœtus was small, probably a female with a small head. Her health had continued, without interruption, good. She thought she never had enjoyed such health as since her pregnancy began.

January 2, 1887, Dr. J. Taber Johnson made a careful examination. We concurred in the decision that she should be permitted to go to term. This decision was accepted with marked pleasure.

Labor began at 5 P.M. on February 8, and terminated successfully at 3:30 A.M., February 9. I arrived at 10 P.M., five hours after the hour named when labor commenced. The os was dilated sufficiently to admit two fingers, and dilatable. The bag of waters was forming but not protruding. The head presented in L. O. A. position. The sutures were easily distinguished. The head was small and flexible. The pains were frequent and sharp, but ineffective. Just at the moment of greatest intensity of every pain violent retching and vomiting would set in, which ceased with shivering. The pains seemed to cease spontaneously with the onset of these complications. The patient and nurse agreed in the statement that the gastric disturbances had continued from the beginning of the pains as I had witnessed them. Her pulse and general condition were good, except that during these attacks her pulse would rise rapidly, sometimes to 120, and then subside during the interval to 80 or 90. There was also a frequently recurring, irresistible desire to pass water. Her courage was unabated, and her cheerfulness and enthusiasm were unimpaired. The bowels and bladder had been freely evacuated before my arrival. The shiverings were arrested by the usual potation of whiskey, which the nurse had refused to allow without my consent. The vomiting not only continued, but manifestly increased in duration and violence, notwithstanding my efforts to arrest it with pellets of ice, alkalies, and hot water. The matter vomited was fluid, very slightly streaked with mucus. The intervals of relief grew shorter and the pains more acutely teasing and worrying. The signs of exhaustion were shown in the more continuous frequency and feebleness of the pulse, varying from 110 to 124, increasing restlessness and anxiety, with a facial expression of suffering and tire, and constant appeals for assistance. From the time of my arrival until midnight there was no appreciable progress in the labor. The relations of the head and condition of the cervix remained unchanged.

With the end of my index finger against the most dependent portion of the bag of waters during a pain, I frequently recognized the simultaneousness of the beginning of the vomiting and subsidence of the uterine contractions, and their correspondence with the moment when the pain or contraction had reached its maximum intensity. If this observation were correct, and the method adopted correctly measured the duration and degree of the uterine contractions, no progress in the labor could be expected during the continuance of the conditions which produced the inertia. Three methods of relief were considered: rupture of the amniotic sac, a rectal injection of chloral hydrate, and anæsthesia. I determined to try the last first and, in the event of failure, each of the others in retrograde succession. At 11:50 P.M. I began the administration of the A. C. E. mixture.

The vomiting ceased; the pulse improved in force and volume, and slowed, and the pains became more direct and distinctly intermitting. After continuing the anæsthesia for three-quarters of an hour it was suspended. The vomiting recurred. The anæsthetic was resumed and not again withdrawn until the head had escaped. The vomiting did not return.

At 1:30 A.M. I ruptured the amniotic sac, and delivery was completed at 3:30 A.M., February 9. The sac was ruptured because its persistence retarded the labor. After the cessation of the stomachal complication nothing worthy of special notice occurred. During the progress of the descent of the head I examined the relation of the head to the exostosis several times. It did not present any obstacle to delivery. There was not upon any part of the child any mark of its impingement. The child, a female, weighed 7½ pounds. Her convalescence was satisfactory, except unusual thirst and hunger during the first day, and delay of milk until the sixth.

The patient was a healthy, robust young woman, bright, intelligent and cheerful, and possessing more than ordinary will-power, self-control and courage. Nevertheless, as admitted since her confinement, she was, from the beginning to the termination of pregnancy, under continuous and very severe mental strain. At times, when disturbed by some perturbating circumstance, such as a visit by appointment from me, the dominating influence of her will would lose its controlling power, hence the rigors and shiverings. These, as well as the persistent vomiting, must be regarded as nervous, perhaps hysterical phenomena. The condition of her health during the period of pregnancy excluded uræmia and every disorder of the chylo-poietic viscera which could have borne any etiological relation to the hyper-emesis. Mental strain with loss of will-power culminated with the onset of labor. The knowledge of the fact that with the beginning of the pains the travail had commenced, to which for nine consecutive months her highest and noblest aspirations, as well as the gravest forebodings, had pointed as the most momentous act in her life, was quite sufficient, even in one so courageous and self-poised, to produce some serious neurotic complication.

The causal relation of the mental condition to the persistent vomiting is affirmed by the fact that obliteration of consciousness, pain and cerebration by narcosis, arrested the stomachal disorder, which recurred upon the restoration of these faculties, but again ceased, not to return during anæsthesia, notwithstanding the contractions of the womb continuously increased in intensity and duration during the same time. The coincidence between the uterine contractions and the vomitings does, however, seem to establish the relation of cause and effect. Sick labors are usually associated with rapid dilatation of the cervix. In this case the os had dilated to two fingers' width (about the size of a silver half-dollar) at the expiration of the seventh hour of continuous labor, and dilated much more rapidly after the arrest of the vomiting.

Vesical tenesmus is constantly found in connection with a rigid os, but in this case it was persistent with a dilatable os, and ceased with the vomiting during narcosis. The slowed dilatation was undoubtedly due to the arrest of uterine contraction by the simultaneous attacks of nausea and vomiting. With a dilatable os there was no protrusion of the amniotic sac, and the amount of liquor amnii was small, so that intra uterine pressure could not be considered a factor unless, perhaps, the small quantity of amniotic fluid permitted closer contact of the uterus with the irregularities of the fœtal ovoid. During the first pregnancy the patient had suffered continuously from nausea and vomiting, but during the second she had been entirely free from any digestive disorder. Thus, while I would not deny the reflex relation between the contractions of the uterus and the stomachal complication, the history of the case, and rather singular array of phenomena occurring during labor, together with the relief obtained by semi narcosis, compel me to reject it as the sole or chief element of causation of the persistent vomiting.

This case is another striking illustration of the value of anæsthesia in labor. The contractions of the uterus were concentrated and increased in power. Usually I do not commence the administration of the anæsthetic before the beginning of the second stage, but I have quite frequently witnessed a similar result when, in consequence of some perturbation, the uterine contractions seem to be ill defined, diffused, and as it were wasted. In very many cases anæthesia expedites labor, in a few it retards it, and in very rare instances it is not well borne, and has to be withdrawn. During the past two years I have most frequently employed the A. C. E. mixture with satisfactory results. It seems to be equally effective in lessening suffering, with less narcosis.

AN UNUSUAL CASE OF LACHRYMAL STRICTURE.

BY H. D. THOMASON, M.D.,
OF ALBION, MICH.

S. C., male, aged 15 years, presented himself at my office two months ago, suffering from, and presenting all the characteristic symptoms of, stricture of the lachrymal duct. There was also a fistulous opening situated below the junction of the canal and duct, making an ugly sore, and discharging tears and pus. This stricture was of several years' standing, and had been annoying, but the fistula was recent, and it was for this that his parents brought him for treatment.

His history was not encouraging—there were strong reasons to suspect hereditary specific taint. He presented an anæmic appearance, had been afflicted with partial hemiplegia since babyhood, and for years had been the victim of severe chronic pharyngeal and nasal inflammation, accompanied with ulceration of the turbinated bones.

I placed him on anti-specific and tonic treatment, and by means of douches and sprays, nasal, post-nasal and pharyngeal, endeavored to modify the difficulty. By means of Anel's syringe efforts were made to pass a stream through the fistulous opening and the lower punctum into and through the duct, but

unsuccessfully. The fistula, however, healed, but an absolute stricture remained. With a set of Bowman's probes, I decided to dilate the stricture. The patient was of such a nervous temperament that nothing could be accomplished unless under an anæsthetic, which was always employed when the probes were used. The lachrymal canal was thoroughly laid open to its junction with the duct, and a No. 1 probe inserted. This was arrested at a point a little below the junction, but by gentle perseverance this point of resistance was passed. Other slight obstructions were overcome, until, when estimated to be within one-twelfth of an inch of the final termination, an obstinate obstruction was encountered, which conveyed the impression it was an obstruction of bone, and which successfully resisted the further passage of the instrument. A No. 2 probe was easily passed to this point and then arrested. A few days later, again placing the patient under an anæsthetic, beginning with a No. 2 probe, Nos. 3 and 4 were successively passed to and arrested at this same point.

Then, after passing all the set of probes, including No. 8, and being unable to get beyond this obstruction, an instrument was ordered and made by Chas. Truax & Co., of Chicago, of the form and diameter of a No. 8 Bowman probe, but armed with a steel drill at the distal extremity. A regular No. 8 probe was passed to the obstruction and withdrawn, in order to open as well as could be the tract, and the armed probe entered. There was a little difficulty experienced in passing it, owing to the disposition of the sharpened edges of the drill to catch in the membrane, but patience and perseverance overcame this, and the obstruction reached, several rotary motions of the drill were made, and there was no difficulty in detecting when the bone was perforated. The drill was withdrawn, a No. 8 probe passed the entire length without difficulty. The patient experiences great relief, and is in a large measure free from the troublesome symptoms attending a lachrymal stricture.

Albion, Mich., April 16, 1887.

A NOTE ON TANNIC ACID AS A SURGICAL DRESSING.

BY T. J. HUTTON, M.D.,

OF FERGUS FALLS, MINNESOTA.

Tannic acid forms an excellent dressing in three classes of wounds, viz.:

1. Incised wounds—applied after the sutures are inserted, or adhesive plaster is on—if the wound does not require stitching.

2. Small wounds of irregular form and recent occurrence.

3. Wounds of moderate size in compound factures. Wherever applicable it excels all other dressings in the following respects:

1. Convenience.
2. Cheapness.
3. Cleanliness.
4. Efficiency.

It is always ready. It costs but a trifle. It requires no greasy mixing, measuring or muddling and has neither smut nor smell. While of its efficiency I can only say after sixteen years use that I am satisfied: I ask nothing better. I first called attention to this neat, choice dressing in an article entitled "Fifty Fractures in Mining Surgery," which appeared in the *Medical and Surgical Reporter* for September, 1878.

The method of application is simply to keep the wound covered with the powder. Wounds thus treated heal on the average in about one-third of the time required for similar ones treated by liquid, oily or salve dressings. In converting compound fractures into simple fractures by this method, the flesh wound is often healed in one-twelfth of the time required to heal it by wet dressings or salves that are frequently removed and re-applied. Illustrations are unnecessary if I have clearly defined the classes of wounds in which this dressing is applicable. Although hundreds of cases thus treated could be adduced that would prove interesting reading, I will describe the last case only in which this dressing was employed:

F. J., aged 16, German, on January 26, 1887, was struck by a falling tree. Next day he was brought to my office. There was a semi-circular scalp wound six inches in length over left parietal bone, and a fragment of this bone almost the size of a silver quarter dollar, stuck up trap-door fashion. The flaps had shrunk wide apart and were swollen and angry, as the injury had occurred twenty hours previously. The whole wound looked bad, being full of hair, dirt, serous effusion, chips, leaves and brush-wood. The patient was very feeble, but conscious. A dose of whisky and hot water was administered and the wound douched for one hour with warm carbolized water. When the bony fragment had been gently depressed almost to normal position, (I could not succeed in reducing it to the normal level) the scalp-wound was sutured with horse hair. Tannic acid was then applied, and directions given that it be dusted on freely every few hours, more especially if moisture should appear over any part of the wound. A laxative was administered, also small doses of aconite and bromide of potassium.

The flesh wound healed by first intention. No fever ensued; the pulse never exceeded 92. The patient went home on the 31st—the fourth day of treatment, and two weeks later his father reported that he was "all right."

April 9, 1887.

MEDICAL PROGRESS.

INFLUENCE OF ALTITUDE ON PHTHISIS.—SURGEON MAJOR NATHANIEL ALCOCK says: The explanations hitherto offered of the action of altitude upon phthisis amount to three: greater expansion of the chest from aerial rarefaction; absence of bacterial organisms from the air and general tonic effect.[1] Were the first of these effective, some improvement would be expected to follow the use of gymnastics at lower levels; of the second it may be said that every consumptive carries his own microbes with

[1] The Lancet, Jan. 8, 1887.

him; and the third is too vague to need discussion. It may therefore be assumed that no satisfactory explanation has as yet been suggested. The specific organism of tubercle has an established identity; consequently the disease must be ranked among the special infections and its aggravation or arrest must depend upon the fertility or sterility of the microbe to which it is due.

Pasteur, in his splendid researches on the poison of splenic fever, has shown that "splenic fever can never be taken by fowls; in vain are they inoculated with a considerable quantity of splenic blood." "Now, the temperature of birds being between 106° and 108°, may it not be," said Pasteur, "that the fowls are protected from the disease because their blood is too warm? A hen was taken, and, after inoculating it with splenic fever blood, it was cooled down to 100°. At the end of twenty-four hours the hen was dead. Again, a hen was inoculated, subjected like the first to cooling, and when the fever was at its height it was wrapped in cotton-wool and placed in an oven at 95°. In a few hours it was fully restored to health. Hens killed after having been thus saved no longer showed the slightest trace of splenic organisms." Thus it is proved that the microbe of one of the most virulent diseases reaches its condition of highest life and greatest reproduction in blood at a temperature of about 100°, but that in the same blood at 107° it ceases to exist. Presumably, then, if the subjects of some forms of bacterial infection could survive a proportionate elevation of temperature, they too would overcome the poison.

The question here arises, Are there any conditions in which the blood of a living man can be made to assume the molecular action equivalent to a temperature of 105° while the animal heat remains at 98°? Clearly such is quite possible by diminishing atmospheric pressure while the temperature remains fixed. We know that it would be quite impossible to boil water at the sea level with 199° of heat, but if the water be removed to St. Gothard, 6808 ft. high, boiling will at once take place. Professor Thomson, in his opening address at the British Association, said: "It is scarcely possible to help anticipating in idea the arrival at a complete theory of matter, in which all its properties will be seen to be merely attributes of motion." It is already accepted that the only difference between the liquid and gaseous states is that of a molecular motion. Consequently, if 199° of heat can make water boil at St. Gothard, it is apparent that this temperature can there produce an amount of molecular motion which not less than 212° could excite at the sea level. Further, as 199° is to 212° so is 98.4 to 104.8°; therefore the rate of molecular motion to which a temperature of 98.4° would give rise at St. Gothard could only be attained at the sea level as the result of a temperature of 104.8°.

Professor Clifford has shown that "the energy of the single particles is always proportional to the temperature of the gas," provided the pressure remains unaltered. Consequently, since the condition of every organism is but the aggregate of its ultimate atoms, if a man whose temperature at the sea level is 98.4° be removed to St. Gothard, it is evident that both the gaseous and liquid molecules in his blood must attain a rate of motion corresponding to what would have been produced at sea level by a temperature of 104.8°. But the experiment of Pasteur has demonstrated that the optimum temperature of certain infecting organisms is about 100°, and that when the vibration-rates equivalent to 106° or 107° are communicated to their constituent molecules, disorganization follows. Hence, it is intelligible that, if a consumptive patient whose temperature at sea level is 98.4° be raised to an elevation of 6000 ft., such alteration of molecular motion will take place in the blood as to be incompatible with the healthy existence and effective reproduction of the tubercular microbe; in fact, the patient will be placed in the position of the bird to the splenic fever poison. If this reasoning be sound, wide is the vista with regard to the epidemic levels of yellow fever, malaria, cholera, etc., which it must open up, and strange the possibilities which might result from the artificial production of the required conditions.—*Lancet*, March 19, 1887.

TREATMENT OF BOILS BY INJECTIONS OF CARBOLIC ACID.—DR. BIDDER, of Paris, has described a method of treating furuncles by parenchymatous injections af carbolic acid. If the boil is a small one, he gives one injection of a few drops of a solution of carbolic acid (2 per cent.); if it is of median size, two injections are given, the half or the whole of a Pravaz-syringeful of the solution being used on each occasion. In the case of large furuncles, for example, half the size of a man's hand, Dr. Bidder injects at four different spots the contents of four Pravaz syringes half or wholly filled with a solution of 2 per cent. of carbolic acid. These injections are given only once. This treatment is strikingly successful. There is some smarting at the seat of injection at first, but the pain soon disappears, and the next day there is a marked improvement in the patient's condition. The inflammatory swelling subsides very quickly, and in eight or ten days even the largest furuncle is dispersed. By this plan no unsightly scars are left, a circumstance, which in many cases is of considerable importance. The success of the treatment is probably to be accounted for by the fact that either the microbes which cause the disease are killed, or the medium in which they flourish is destroyed—*British Medical Journal.* [This treatment has been in use in America for almost ten years.]

HYPNONE.—Von Schüder writes of fourteen patients treated by hypnone, in whom favorable results followed. A dose of from 2 to 4 drops was sufficient to produce sleep of several hours' duration; the effect was especially happy among the phthisical. No ill effects were observed. In one case only, after 6 drops had been given, the patient awoke from a long sleep with headache and slight vomiting. The effect dependent upon the dose and individual peculiarities was manifested after from one-half to one and one-half hours.—*Der Pharmaceut*, Feb. 1, 1887.

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THE EDITOR OF THIS JOURNAL would be glad to receive any items of general interest in regard to local events, or matters that it is desirable to call to the attention of the profession. Letters written for publication or containing items of information should be accompanied by the writer's full name and address, although not necessarily to be published. All communications in regard to editorial work should be addressed to the Editor.

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THE IRRADIATION OF MOTOR IMPULSES.

If a man perform work with the muscles of, for example, his right hand exclusively, and to the point of fatigue, can he thereafter perform as much work of the same nature, with the left hand as he could if the right hand had not been previously exercised? Such is the question discussed by DR. N. A. RANDOLPH before the College of Physicians of Philadelphia on March 2. The question does not relate to a comparison of the work of the two hands, but to an examination of the work which may be done by one hand as conditioned by the previous exercise or non-exercise of its fellow; and it is clear that the answer to question depends solely on intracranial processes, and that the answer would throw light on the functional independence or interdependence of the two halves of the brain. And to answer the question two conditions are prerequisite: the subject of experiment must be ignorant of the object of the investigation, as otherwise he will be unconsciously a partisan of one hand, and there must also be a strong inducement for him to exercise his volition to the utmost.

Suitable subjects were found in the persons of some of the more vigorous and intelligent convicts in the Eastern Penitentiary, the stimulus being a money prize to the one who performed the most work in a given time. In the first series of experiments rubber-bulb syringes, identical in all their measurements, were used, the work performed being estimated by the amount of water which the men could transfer from one vessel to another in a given time. The uniform result of some forty observations was that either hand could do more work when its exercise preceded than when it followed the similar exercise of its fellow. "It was found, however, that the muscular effort could not be entirely restricted to one side of the body in this method, as great fatigue was always accompanied by a grimacing and writhing which implicated the muscles of both sides of the face and trunk." In the next experiments a Morse telegraph instrument was used, the muscular movements being restricted to an up-and-down motion of one finger of each hand, the number of such movements in a given time being recorded on the telegraph slip as dots or dashes, in accordance with the rapidity of contraction and relaxation of the flexors of the fingers. The results of these experiments were uniformly confirmatory of those in the first experiment; but the method caused such eye-strain in counting that it was abandoned. A clock-work instrument was then made, so that the records could be made on a dial and easily noted. Six subjects were used, and to each was given fifteen minutes in which to make his record with, for example the right fore-finger, after which the left fore-finger was similarly exercised. The same process was repeated on the following day, but beginning with the finger of the hand used second on the previous day.

These results were also practically uniform. "The man who for fifteen minutes flexed and relaxed his right forefinger with the greatest speed possible to him would, on the following day, accomplish on an average, nearly 10 per cent. less work with that finger when its exercise was consecutive to a similar exercise of the forefinger of the opposite side, than when its work was initial." As a rule, more work was accomplished by the simultaneous exercise of the two forefingers than by their successive exercise. In such simultaneous exercise of both hands, apparently from some unconscious effort at rhythm, it was seen that the movements of the left forefinger were generally more active and rapid than when used alone. This is in accordance with experience of many pianists. It will be seen that these facts are closely related to the observations of Weir Mitchell and Morris Lewis in regard to the knee-jerk. It will be remembered that they have shown that the knee-jerk is reinforced by any voluntary movement in any part of the body, and that this reinforcement is apparently due to such an irradiation of motor impulses from the active centres to other similar centres, as placed them and their related muscles in a condition of heightened responsiveness to external stimuli. Dr. Randolph's experiments seem to confirm this, and show that fatigue of one centre may induce a sympathetic fatigue in other centres. These

observations further suggest that the centres for volition, attention and coördination are not, as regards their functional activity, bilaterally symmetrical and independent; that is to say, "these functions have not attained complete differentiation into right and left will, attention, or coördination; that, probably, the first effect of the voluntary activity of a portion of one cortical motor area is a stimulation of the corresponding portion of the other hemisphere—a stimulation that may result in its slightly premature fatigue; that apparently more work can be effected through the voluntary simultaneous exercise of two such portions of the motor apparatus than by their independent exercise one after the other."

Can we explain Dr. Randolph's facts by saying that the overflow of energy on to symmetrically related centres, or on to others, is competent to weaken them without being strong enough to cause motion? Dr. Weir Mitchell inclines toward the use of the overflow theory to explain the lowered capacity for work by one hand after exhaustion of the other. It will explain why in consentaneous use of two symmetrical parts more work is done than when they follow one the other. The overflow would be in this case valuable, and not damaging or wasteful. He referred to the fact, which he and Dr. Lewis have established, that "When we use the maximum power of one hand on a dynamometer, the coinstantaneous use of the other hand adds nothing to the result; and this form of experiment has been commonly used as a test of the reinforcing capacity of the opposite member. If, however, using two fingers, or the grip of the thigh adductors, on the bulb of a mercural dynamometer until great exhaustion occurs, and we then make a new effort at the moment of violent use of another member, the mercury leaps quite to the level attained during the first effort by unfatigued muscles. It does not seem easy to explain this fact, except by assuming that the overflow of energy usually wasted is in this case made efficient."

This experiment brings up the question of muscular and ganglionic tone. When we strike the patellar tendon, a sudden, distant, voluntary act adds reinforcement. What happens to the muscle or ganglion so influenced? Is it made more sensitive to impressions, or is there with this a slight flow upon it of motor energy? And if so, can we measure the effect, and thus influence what we conceive of as muscular tone? Dr. Mitchell has been engaged in discovering if these reinforcements do cause motion —*i. e.*, a slight preparatory muscular contraction making the subsequent volition, or other excitatory activity, more potent in its results. He has been able, so far, to prove that in some spastic cases distant muscular effort, such as a grimace, really causes distinct and measurable movement in the extensors of the thigh and presumably elsewhere. But it is not yet clear whether in *normal* man remote motion is thus capable of causing slight shortening of all other muscles. We often speak, says Dr. Mitchell, of nerve power as if there were a common stock from which are drawn the supplies needed by every active organ, and reason that it is unwise to try to carry on at once two functions which exact large expenditures—as digestion and intense thought, or digestion and exercise. Practically the difficulty may be one chiefly of blood supply. This is illustrated in the not rare fact that some feeble people cannot digest except when at rest. These facts suggest the idea that perhaps Dr. Randolph's cases would lose 10 per cent. of mechanical capacity after a period of exhausting mental labor or during digestion.

In regard to the old theory for explaining the principle of counter-irritation, that there is a certain amount of nerve force in the system, and that when by means of counter-irritation the nerve force is drawn to a distant point, it is removed from the inflamed part, we may agree with Dr. H. C. Wood that, while modern science does not recognize the truth of this theory, it looks as though there is a certain amount of truth in it. "Every one who has worked in a gymnasium will recall the fact that he cannot use the two hands simultaneously with the same force as he can when the two hands are used separately. This shows some relation between the nerve centres which we have not as yet got at." It would seem that when we use our muscles vigorously, two kinds of fatigue are produced; a local fatigue and a general fatigue. If a man uses the right arm vigorously, he not only fatigues the right arm, but also the whole body. Dr. Wood believes that if these investigations are continued, it will be found that after prolonged use of the leg, there will be loss of power in the arm, perhaps as great as after previous use of the other arm. As suggested by Dr. Mitchell, "the relation between muscular exertion and mental exercise should be studied. Each one knows by personal experience that when mentally fatigued he is incapable of performing the usual amount of physical labor. This is probably independent of any question of overflow, and goes back to the higher cerebral centres and their relation." As suggested by Dr. Mills, it is a well-known clinical fact that an old hemiplegic, if examined carefully, will be found to have not only the decided loss of power and

accompanying conditions resulting from the lesion on the opposite side of the brain, but also a certain diminution of strength in the limbs of the other side; a condition which is not entirely due to the general loss of physical power present. The phenomena exhibited in certain cases of unilateral spasm seem also to be related to this subject.

Experiments such as those of Dr. Randolph are not only of great interest in connection with pure physiology, but also in connection with psycho-physiology, or physiological psychology, which is now receiving more and more attention, and on which Prof. Ladd, of Yale, has recently written a most valuable book.

ANNUAL REPORTS OF PUBLIC HOSPITALS.

The twenty-sixth annual report of the Cincinnati Hospital, which has just been laid on our table, reminds us of the many and great advantages that would result, if all public hospitals for the sick and injured were required to make full annual reports, for which the one before us would afford an excellent pattern. It is a neatly printed pamphlet of only 78 pages, yet contains a concise description of the hospital buildings; full list of the officers, medical staff, resident physicians and nurses; complete financial statement of all receipts and expenditures, including every item of quantity and cost; the number of admissions and discharges, medical, surgical, obstetrical and gynæcological, with the particular disease or accident affecting each patient, and the results of the treatment; and also the total number treated during the year, the average time in the hospital, the total cost and the average cost per day per patient. If every public hospital containing fifty or more beds would publish annually a complete official report on the model given us in the twenty-sixth annual report of the Cincinnati Hospital, we would soon have the materials and data for solving many important problems, both economical and medical much more satisfactorily than at present. Enough can be gleaned from the few, and often important, reports of hospitals now furnished to the public, to show great differences in the expenses per patient in different hospitals, even when located in the same city, and apparently receiving the same class of patients; and almost as much disparity in the results of treatment, both in the general aggregate and in relation to particular diseases. Thus in one hospital the total cost per day for each patient is given at about $1.20 and in another at only 0.67\frac{7}{10}$. In the report of the Cincinnati Hospital the net cost per patient per day is given for each year from 1868 to 1886 inclusive, and varies from 47.7 cents to 72.6 cent. The percentage of deaths to the whole number of patients was 6½, that of typhoid fever alone nearly 17, and that of pneumonia 45 per cent.; while in another hospital the ratio of deaths from typhoid fever is only 5 per cent. and from pneumonia less than 15. A series of official reports from the public hospitals of different cities would enable us to explain these important differences, and deduce practical lessons of much value.

AMERICAN PUBLIC HEALTH ASSOCIATION.

The preliminary circular announces that the next annual meeting of this important organization will be held in Memphis, Tenn., November 8, 9, 10, 11, 1887. The Executive Committee have selected the following topics for special consideration at the meeting: *a.* The Pollution of Water-Supplies; *b.* The disposal of Refuse Matter of Cities; *c.* The disposal of Refuse Matter of Villages, Summer Resorts and Isolated Tenements; *d.* Animal Diseases Dangerous to Man. No more interesting or important topics could be selected for the thorough study of the Sanitarians of this country at the present time. All papers intended for this meeting should be plainly written or printed, and be in the hands of the Secretary at least twenty days before the date of the annual meeting. Notwithstanding the enumeration of the topics mentioned, it is understood that papers of merit on other subjects will not be excluded. George M. Sternberg, U. S. A., Baltimore, is President, and Irving A. Watson, Concord, N. H., Secretary.

AMERICAN SURGICAL ASSOCIATION. — The next annual meeting of this organization is to be held in Washington, D. C., commencing May 11, 1887, and is expected to continue three days. The preliminary programme contains a list of many papers, furnishing interesting topics for discussion. Hunter McGuire, M.D., Richmond, Va, is President, and J. R. Weist, M.D., Richmond, Ind., Secretary.

STATE MEDICAL SOCIETY OF WISCONSIN.—The next annual meeting of this Society is to be held at Oshkosh, commencing on Tuesday, May 3, at 8 o'clock P. M. Reports of such a number of Standing and Special Committees are expected as to make a full amount of interesting and profitable work for the meeting.

SOCIETY PROCEEDINGS.

MEDICAL SOCIETY OF THE DISTRICT OF COLUMBIA.

Stated Meeting, March 2, 1887.

THE PRESIDENT, JOS. TABER JOHNSON, M.D., IN THE CHAIR.

DR. S. C. BUSEY read a paper entitled

PERSISTENT VOMITING DURING LABOR RELIEVED BY ANÆSTHESIA.

(See page 484.)

THE PRESIDENT stated that he had seen the cases reported by Dr. Busey and would confirm his statements. He suggested that in addition to the subject for discussion mentioned in the paper, the subjects—"Exostosis as a Complication of Labor," and "Anæsthesia in Labor" be discussed.

DR. L. MACKALL said that he had recently attended a case of labor in which the use of anæsthetics had not only proved efficacious in relieving very distressing nausea and vomiting but also promoted a speedy delivery. He was called in January last to see Mrs. C—in her second confinement. Arriving between 10 and 11 o'clock P. M., he found the os dilated to the extent of a silver dollar, and dilatable; the bag of waters protruding; the vagina relaxed and the pains recurring at short intervals but not expulsive. Up to this time nausea and vomiting were not present, and from the favorable state of the patient he anticipated a speedy delivery. She soon began to complain of sick stomach, which became distressing, with frequent retchings and diminished frequency of pains.

He found upon examination about an hour after his arrival that labor had not only not advanced but that the soft and dilatable os had become rigid and unyielding. During the next two hours there was much sick stomach and frequent emesis of mucus and water. The pains were diminishing in strength, were not all expulsive. Between 2 and 3 o'clock A. M. he made another examination and found that there had been no advance in labor and that the os was quite as rigid as before.

He felt convinced that the irritable stomach and the tardy labor were due to a too great determination of nerve force to the solar plexus, thereby depriving the hypogastric plexus of the requisite amount of force necessary for the accomplishment of labor, and that in order to overcome this abnormal state he had either to administer by the stomach some agent capable of relieving the overcharged nerve center, or would have to resort to an anæsthetic, which by its action on the cerebro-spinal system, would probably withdraw nerve force from the solar plexus to the cord and that then a due determination to the hypogastric plexus would take place. He resolved, in order to accomplish the desired end, to try first a cupful of crushed ice given as rapidly as could be swallowed, as he had previously learned that ice so administered not only allayed gastric irritability, but quickly promoted efficient uterine contractions and prompt delivery. In this connection, he felt it incumbent upon him, to again make special reference to the good effect of ice when properly administered in labor attended with inertia, and he was satisfied, that many cases occurred where, if it had been administered, the use of forceps might have been dispensed with. He could recall a case where uterine contractions had ceased almost entirely, although large and repeated doses of ergot had been given, and the application of the forceps seemed inevitable, but before using them, he gave a large teacupful of ice as rapidly as could be swallowed, with the result of restoring efficient pains and a rapid delivery.

In the case under consideration, however, the crushed ice failed to accomplish a good result, so he determined to put the patient under the effects of the "A. C. E." mixture. Six or eight inhalations, during pains, reduced her to partial unconsciousness.

After the first inhalation there was no recurrence of the nausea or vomiting, and the character of the pains quickly changed, becoming strong and expulsive. The os was found to have lost its rigidity and to be rapidily dilating, and in a half hour the child was born. Contraction of the uterus promptly followed with expulsion of the placenta without hæmorrhage.

In referring to a statement made in Dr. Busey's paper, that nausea and vomiting were regarded by many as favorable to the progress of labor, Dr. M. stated that this might be true if the vomiting resulted from reflex action due to a rapid dilatation of the os, but that in those cases this condition should be regarded as the effect not the cause. In other cases where the nausea, vomiting, pyrosis, etc., resulted from great irritability of the stomach, caused probably by a sudden determination of nerve force to the solar plexus, he had found that no favorable or decided progress was made until these symptoms were relieved by appropriate treatment. Indeed he rather dreaded such cases, for he then anticipated that probably many hours must elapse before the patient and doctor would be relieved.

DR. L. ELIOT related a case also attended by vomiting. When called he found the os dilated and every evidence that the labor was progressing. After trying various remedies without effect, tincture of iodine finally controlled the vomiting, but the pains ceased. He had her sit up in bed for awhile and in two or three hours she was delivered.

DR. J. F. HARTIGAN has used anæthesia in cases of tedious labor with satisfaction. We should be cautious in its use and the selection of the anæsthetic. For instance, the primary effect of chloroform was upon the heart, while ether is known to have produced hæmorrhage from the air passages and lungs. The "A. C. E." mixture, he thought, was the safest and most popular.

DR. L. MACKALL referred to the recently published article of Dr. Fordyce Barker upon the subject, in which he claims that chloroform is perfectly safe.

THE PRESIDENT remarked that he preferred chloroform in labor, and that he used it in perhaps seven-tenths of his cases. He only used it, however, ex-

cept in operative procedures, when the patient asked for it, and he began giving it in the latter part of the second stage. He puts twenty to thirty drops on a handkerchief and allows the woman to inhale it herself just as a pain is coming on. In this way she gets enough to dull her sensibility to pain, but not enough to keep her unconscious between whiles. In a large number of cases there may be a few instances in which anæsthesia has retarded labor enough to make the forceps necessary, but he does not think that it will produce post-partum hæmorrhage. On the contrary, the restlessness and exhaustion produced by a long labor without relief is much more likely to cause hæmorrhage than the chloroform. He prefers chloroform to ether in heart or kidney disease.

Dr. T. C. Smith thought that anæsthesia did sometimes help post-partum hæmorrhage.

Dr. Busey has seen hæmorrhage follow anæsthesia, but he did not attribute hæmorrhage to it, but rather to protracted and exhausting labor. Chloroform was safer than ether, but the "A. C. E." mixture was superior to any. Narcosis was not so profound as by the former while it is just as effective in relieving pain. Although anæsthesia is a great boon in midwifery, it may sometimes prolong labor instead of expediting it.

Stated Meeting, March 16, 1887.

The President in the Chair.

Dr. D. S. Lamb presented the specimen and read the history of

A CASE OF OSTEOMALACIA.

Dr. Mary Parsons reports the following history: Charlotte J., white, single, Virginian, seamstress. In November, 1865 (she was then 47 years old), while walking home one day, she was suddenly seized with severe pain in dorsum of left foot; this was found to be swollen and reddened. The pain continued through the night. The late Dr. W. B. Magruder was called in the morning. After careful examination he failed to find the cause of the trouble. The pain, redness and swelling recurred at intervals for two years, sometimes preventing sleep and finally interrupting her work. In the autumn of 1867, after an unusually severe paroxysm, lasting several weeks, she fell and sprained her left ankle, for which she was treated by the late Dr. S. B. Blanchard. She kept her bed several months and never resumed her regular occupation, but did some sewing at her house. She was able to get about the house with comparative ease and sometimes walked short distances. About a year later she had a second fall and another severe sprain of the same ankle, after which she walked with much difficulty and had constant pain. In 1872, unable to earn her living, she was admitted to the Home of the Women's Christian Association. She then walked with a cane, waited on herself and took care of her room. November 15 she was sent to Columbia Hospital. She had then constant pain in her left leg and ankle, pain along course of posttibial nerve, and some œdema about the ankle. Treatment seemed of no benefit. Dr. J. Harry Thompson removed a portion of the nerve at the seat of the severest pain. Wound healed by first intention, but no relief followed. She returned to the Women's Home. She now went on crutches and with great difficulty; pain more severe. Six weeks afterwards she had an unusually severe paroxysm of pain in the foot, which was reddened and swollen; she was put to bed and never left it again in fourteen years. During this time she had constant and severe pain, sometimes excruciating; she became emaciated and anæmic; the toes of the left foot became flexed, finally causing ulceration of the under side of the foot; foot œdematous, œdema extending to knee. Diet generous, her fancies gratified, but she never improved in her general condition. She often had "drawing" pain in the foot, and just before a storm extending to the thigh; and spasm of the leg which gradually diminished as the disease progressed. For several years she did fine needlework and depended on conversation for pastime, being unable to read. About 1881, she had irido-choroiditis of left eye, which recurred sometimes in one eye, sometimes in the other, and at times in both. In December, 1885, an ulcer appeared on the right cornea, and perforated; eye removed by Dr. S. M. Burnett. During these years the pain steadily increased, the foot rotated outwards, and in 1885–6 the leg rotated outwards at the knee. The œdema increased and the limb became a shapeless mass. For the last six months of life she could not lie down because of the "drawing" pain. Her pains increased much in stormy weather, and this led many physicians to diagnosticate rheumatic arthritis. Her appetite became capricious, constipation obstinate, urine scanty and loaded with urates. Through her later years she was a marvel of patient endurance. Died June 15, 1886.

Necropsy by Dr. D. S. Lamb: Body much emaciated; bedsores on buttocks; right eye had been removed. Right lower limb œdematous; left, flabby; linear white scar on inner side of left ankle; eczematous scabs above ankle; muscles of limb showed fatty atrophy; bones of thigh, leg and foot atrophied, the atrophy increasing from above downwards. The compact substance was thin, porous and very friable; in some places so soft as to be cut with a knife without turning its edge; the cancellous substance appeared as plates and fibres loosely held in a mass of white granular matter, largely composed of fat, in some places of a gelatinous appearance, filling all the medullary canals and spaces. Brain showed increase of subarachnoidal fluid; cysts of choroid plexus. Old pleuritic adhesions both sides, especially at apices, under which was fibroid induration; both lungs showed red and gray hepatization in lower lobes. Heart normal: aorta and pulmonary artery congested on pericardial surface; aorta showed fatty degeneration. Liver somewhat atrophied; gall bladder full. Spleen somewhat atrophied. Pancreas normal. Stomach normal. Small intestine full of flatus. Large intestine contained much soft fæces which excessively distended the rectum. Right kidney cirrhotic; surface showed many minute aqueous cysts and about

a dozen prominent yellowish masses, size of pinheads and larger, which on section turned out granular matter; at lower end of kidney was a sarcoma, size of small walnut, encapsulated, and slightly encroaching on kidney tissue; mulberry calculus, weighing 10 grains, found in pelvis. Left kidney cirrhotic; small superficial aqueous cysts. Right suprarenal capsule of usual size; left not noticed. Bladder normal. Uterus and appendages atrophied. Lower aorta and common iliac arteries showed chalky plates. Left external iliac artery normal; no obstruction. Lumbar glands normal.

The history and necroscopy of this case correspond so closely to those of mollities ossium or osteomalacia that I am inclined to that diagnosis.

The disease is very rare. Dr. Fagge, in his "Principles of Medicine," 1886, states that he had seen but one case, and that in the care of his uncle, Mr. Hilton, at Guy's Hospital, in 1864. Ten out of every eleven cases occur in the female sex, and two out of every three in women, either during pregnancy, shortly after parturition, or about the time of the climacteric. The age is usually between 25 and 35, rarely under 20 or over 50. The cause is speculative, although many cases seem to have a dependence upon dampness. The earliest symptom is pain, varying in seat and character, sometimes "wandering," and then leading the physician to the diagnosis of "rheumatism." Lassitude, irregular appetite and constipation are usual symptoms. Gradually locomotion becomes difficult and the patient is very liable to fall. In consequence of falls, and, indeed, sometimes even on slight motion, fractures occur, with little or no tendency to heal, and accompanied, therefore, by deformity. Eventually the patient is confined altogether to bed, becomes emaciated, exhausted, and dies. The duration of the disease is usually four to six years, but cases are recorded of its terminating in death in three months, and again lasting for eight, ten, or even fifteen years; in the case now reported, nearly twenty-one years. I see no reason why it may not endure for any period within the ordinary limit of life.

Why the bones should be thus decalcified and softened, so that they can be readily cut with a knife, is not yet clear. Lactic and carbonic acids both have been assigned as the causative factors. No excess of phosphates or carbonates of lime is found in the urine except incidentally; although, as in this case, calculi are found in the kidney and bladder in many cases. The inorganic portion of the bone may be reduced to 30 per cent., the diminution in lime being much more relatively to that of its combining acid. The compact tissue may become a mere shell; the cancellous tissue sometimes seems like medulla, of a deep red color with ecchymoses; sometimes yellow, soft and fatty; sometimes mucous and translucent. This softening may cause deformity of the female pelvis and thereby difficult parturition; and the softening of the ribs may be so extreme as to stop respiration. The disease differs from rickets in this: rickets is due to a failure to deposit the calcareous salts; osteomalacia to the removal of the salts after deposition.

DR. E. O. SHAKSPEARE, of Philadelphia, then addressed the Society upon

SOME NEW ASPECTS OF THE CHOLERA QUESTION SINCE THE DISCOVERY BY KOCH OF THE COMMA BACILLUS.

(See page 477.)

CHICAGO GYNÆCOLOGICAL SOCIETY.

Stated Meeting, Friday, February 18, 1887.

PHILIP ADOLPUS, M.D., IN THE CHAIR.

(*Concluded from page 472.*)

DR. A. REEVES JACKSON exhibited

TWO DERMOID CYSTS OF THE OVARY.

Case 1.—First seen by me May 27, 1884. Julia C., aged 40 years, was married at twenty, had one child two years later, and no other pregnancy. Commenced menstruating at 17, and was regular after the first year. Always had good health. Five month ago, weighed 176 pounds. Three and a half months ago she took a long walk in the evening, while menstruating. After her return she had a slight chill, and severe pelvic pain, the latter chiefly referable to the bladder, micturition frequent and painful. She seemed never to recover her health; vesical symptoms continued, appetite and nutrition failed; she became rapidly emaciated; a swelling appeared in the lower abdomen; menstruation appeared regularly and without pain. She could not walk, but rode out in a carriage. A few days before I saw her she rode out, with enjoyment, but returned fatigued and soon began to vomit. Her upper extremities moved involuntarily, and there was a complete loss of power in the lower ones. A "metaphysician" was called to see her and gave the assurance that there was no bodily ailment—the mind only was at fault. The assurance was repeated twice a day by the Christian scientist, who on these occasions sat out of sight of the patient, the headboard of the bedstead separating them. However, the patient and her friends thought she was getting rapidly worse, and I was asked to see her. At the time of my visit she was lying in bed on her back. Her hands and arms were in constant motion; she seemed powerless to keep them still a moment; she could make no co-ordinate movements, indeed had no control whatever over either the upper or lower extremities; the latter were, however, motionless. The functions of the bladder and bowels were voluntary; the tongue dry and covered with a brownish coating; there was great thirst; pulse 110, small and almost imperceptible at the wrist. The temperature was not observed. She was very restless, and had not slept more than ten minutes at one time during the past three days. At short intervals she vomited a greenish, frothish, tenacious fluid. On examination I found the abdominal walls soft and free from tenderness on pressure. There was a feeling of doubtful fluctuation with dulness at the sides, also in the right iliac region. The higher parts of the ab-

domen, as the patient lay on the back, were resonant. She died the following day, and in the evening an autopsy was made with the assistance of Drs. Brower, Danforth and E. Ingals.

On opening the abdomen we found besides other evidences of acute inflammation the exudation of perhaps a quart of pus. As this was being removed it was discovered that its source was a partially collapsed cyst, which still held about one pint of pus, and from the opening through which the pus came there protruded a few hairs. This fact settled the diagnosis. The woman had died of a ruptured dermoid cyst. The kidneys showed evidence of chronic disease. The cyst with the ovaries and Fallopian tubes, was removed. Here I show you the opening, which was somewhat enlarged after the autopsy for the purpose of freeing the inner portion of the cyst. In addition to the hair there were also some rudimentary teeth and some plates of bones which I sent to a pathologist for examination, and received from him a photograph showing the bony constituents of the cyst. Apparently all of the dermoid tissue was centered in this space. The amount of hair was large, as you see. This was matted together in the usual manner, and has been freed from the sebaceous matter which accompanied it by shaking it in ether. This is a miniature switch made by an artist in such matters from a portion of the hair. These dermoid cysts, of which I have now seen three, possess great interest to me, pathologically. I confess to not understanding them, so I asked Professor Fenger to be here to night, and he promised to bring some additional specimens and to talk about them.

Case 2. Dora B., 35 years old, wife of a physician residing at Normal Park. Saw patient on February 10, 1886. Married fourteen years; two children, age respectively 11 and 7 years; also two abortions, one three years and the other two months since. Nine months ago, while lying on her back, patient noticed a swelling the size of an orange in the right iliac region. It was soft, smooth, movable and insensitive; has slowly encroached on the opposite side; five days ago, after sneezing, experienced a sharp attack of pain in pelvis which lasted twenty-four hours, keeping her in bed. I found on examination an abdominal tumor occupying the hypogastric and right iliac region, extending upward as high as the umbilicus, and into the left iliac fossa. On February 23, she entered my Infirmary, and the tumor was extirpated on the 25th. It weighed about eight pounds. There was a main cyst, containing a limpid serum, and inside of this a smaller cyst about the size of a mandarin orange, containing bone and hair. This tumor possesses an interesting feature that I have never observed before in either of the cases I have seen. It is this, there are two distinct kinds of hair, one of an auburn color, very curly, portions of which are still attached to the part, and two tufts springing from other portions of the rough, orange-rind like skin, which are long and straight, quite free from curl, and of an entirely different color. The kinds of hair that I have seen from other specimens, while they have usually differed in color from the patient's hair, have always been uniform throughout. In the first case I saw, several years ago, the hair was thirty-six inches long. In that case, the part from which the bony substance and hair sprang were in one part of the tumor, occupying a small space. The patient had carried this for many years without symptoms. Finally, it commenced to grow and formed a tumor sufficiently large to attract her attention. There were two cysts. The first one was a very large one, containing a semi colloid matter very much resembling soft soap, and in great abundance. Separate and distinct from this there was a cyst not larger than a goose's egg, soft, and which could be indented readily, and when it was opened there came out a mass of hair together with other dermoid characteristics. So it seemed that the cyst which contained the fluid and made the growth perceptible was secondary, and this fact would account for the clinical fact of these patients carrying a tumor for many years without any impairment of health, until an additional cyst is formed, that may present any of the characteristics of an ordinary ovarian cyst.

Dr. Christian Fenger made some remarks on

DERMOID CYSTS OF THE OVARY,

with illustrations from specimens:

In entering upon the question of the dermoid cysts of the ovary, I wish to call attention to the two theories of their origin. According to Heschl, dermoid cysts in general owe their origin to isolated islands of the epiblast, displaced during embryonal development and located somewhere in the territory of the mesoblast. This theory of fœtal inclusion did not explain the origin of the dermoid cysts in the testicle and ovaries. It was not until his had shown that the internal genital organs are developed from a part of the embryo, the so-called "axenstrang," in which all the germinal layers are included, that we were able to understand the presence of dermoid cysts in those genitary glands.

The second theory of the origin of dermoid cysts in the ovary is the view of the older authors, recently adopted by Waldeyer. Epithelial cells of the ovary, capable of transformation into the ovum with all its formative possibilities, may enter into an irregular formative activity and produce a dermoid cyst—a process almost analogous to a partheno-genetic development, as Ohlshausen states it. This second theory would only explain the origin of dermoid cysts in the ovary, and would not enable us to understand their presence in all other parts of the body. Consequently, it seems more natural to accept the Heschl-His theory, as this gives a satisfactory explanation of the origin of dermoid cysts in general, and is in conformity with Cohnheim's theory of the origin of all other new formations, from an isolated group of embryonal cells, dormant until the unknown cause of the new formation calls them into formative activity.

A dermoid cyst is always a monocyst, and if, as is seldom the case, we find more than one in the same ovary (Ohlshausen in one case found three), we may expect to have had more than one embryonal matrix,

from each of which a cyst has developed, the one independent of the other. It often appears as if a dermoid cyst of the ovary were a multiple one, but closer examination will prove that we have before us a combination of a dermoid cyst and a proliferating cystoma, or more rarely a dermoid cyst with multiple local colloid degeneration of the stroma of the wall. Cystic transformation of the sweat glands —extensive cysts to the size of a fist—was seen in one case by Friedländer.

I shall not go any further into the subject of the dermoid cysts here, but only present to the Society three specimens removed by laparotomy within the past year, and will call attention to the points of interest illustrated by each one in particular.

Case 1.—This specimen, at the time of the operation the size of a fist, now much smaller from shrinking in the alcohol, was removed from a girl of 20. There was no difficulty about the removal, but I am sorry to say the patient died from acute sepsis thirty-six hours after the operation. Besides the sebaceous matter and the hairs, which you have already seen in Dr. Jackson's specimens, we find in dermoid cysts very commonly—in from 20 to 50 per cent. of the cases—teeth inserted in the soft dermoid wall or in pieces of bone contained in the latter, or finally, free in the contents of the sac. As a rule, there are only a few teeth in a cyst; but Schnabel has seen, in a case of a girl of 13, over 100, and Autenrieth describes a case in a 22-year-old woman, in which 300 teeth were removed and as many more left in the cyst. As Ohlshausen states, it is impossible to understand the presence of such numbers of teeth without the explanation that, the same as in children, multiplication of the enamel germ takes place, and a set of milk teeth are followed by a set of permanent ones. That this is more than a mere theory is proved by a specimen in Rokitansky's collection in Vienna, in which there is seen a milk tooth with the root absorbed down to the crown by atrophy from pressure of the overlying permanent tooth. Spencer Wells, in his "Ovarian Tumors," states that he has seen one similar instance. In the specimen before us this fact is illustrated to perfection. From the soft parts on the surface of this little piece of bone, in the wall of the cyst, you see attached a tooth corresponding in shape and size exactly to a temporary incisor of the upper jaw. I have made an incision through the gum, if we may use that expression, and, as you see, the root is absorbed almost down to the crown. When we turn this milk tooth to the side, we see the crown of the overlying tooth. This is larger, and has the exact shape of the corresponding permanent incisor.

Case 2.—The next specimen is a very large dermoid cyst, from the left ovary of a woman at 50. It filled the whole abdominal cavity up into the cardia and gave the exact symptoms of a proliferating cystoma or multiple cyst, as there were felt, besides the main cavity, harder, lobulated portions, which I supposed to be smaller and more tense cysts. As she gave the history of a cyst which ruptured when she was 14 years old, causing months of suffering from peritoneal symptoms and then disappeared, not to return until after the age of 45, I thought that a dermoid cyst was out of the question. At the operation, which was difficult on account of many adhesions and the nature of the contents of the cyst, I found this very large dermoid cyst, containing—(a) Three or four gallons of a brownish fluid, in which floated hundreds of thousands of round, yellowish-white, small bodies, the size of a hemp seed up to a pea. I pass round a sample of them in these two glasses. These bodies are soft, have the consistency of butter and are found under the microscope to consist of irregular masses of amorphous fat, with pavement epithelial cells interspersed here and there, single or in groups; (b) A yellowish-white, butter-like mass the same as the small bodies if matted together, filling up entirely some of the chambers of the cyst, with no fluid mixed with it. This peculiar arrangement of the fat is rare. Rokitansky saw in a cyst seventy bodies the size of a hazlenut, and very many the size of a pea swimming in a brownish fluid. Routh, according to Spencer Wells, saw a similar case, the balls showing under the microscope concentric layers of amorphous fat around a nucleus of cholesterine crystals. Franekel, cited by Ohlshausen, found the whole contents of a dermoid cyst to be numerous hard, mostly round or irregular balls, consisting of amorphous fat, fatty degenerated epithelial cells and hairs. The shape of the cyst is peculiar, inasmuch as it gives the appearance of a conglomeration of cysts, But close inspection shows that all of these communicate with each other so as to form one large, though very irregular cavity. Thus, in reality, we have before us a monocyst, characteristic of the dermoids, as I mentioned before. In the wall, however, we find a number of smaller cysts the size of a pea to a hazlenut—these do not contain the same fatty material as the main cyst, but a colloid mass, and are due to secondary colloid degeneration in the wall of the latter. The inner surface of the large dermoid cyst shows in some places irregular masses of bone imbedded in the wall, and further, as in Dr. Jackson's cases, the following condition: We do not find typical skin with hairs, sebaceous glands, epidermis, and so on everywhere on the inside. This is found on only part of it, forming one or several irregular islands. The remainder of the cyst wall is smooth, has the characteristics of an ordinary cystoma, with a single layer of epithelial, cuboid, or cylindrical cells. It may be that the dermoid portion of the wall secretes the fat and the cystoid portion mainly a serous or albuminous fluid. Movements of a cyst containing at the same time a thin serous fluid and sebaceous matter might (Rokitansky) shape this suspended fat into the small round masses just the same as butter when in the process of churning. However, if this was the right explanation, it appears, that this peculiar formation is seen only in very exceptional cases. The right ovary was transformed into a dermoid cyst the size of an orange. Notwithstanding the dermoid cysts on both sides, the woman had a number of children, the youngest 16 years old at the time of the operation. The patient never rallied from the shock of the operation, and died twelve hours afterward.

Case 3.—The third specimen is a dermoid cyst taken from a girl of 23. It was noticed for about one year and a half before the operation, at which time it was one and a half times as large as a child's head. There was no particular difficulty about getting it out. When I opened the abdomen and came on the cyst it was transparent, so that I did not think it was a dermoid cyst, and I inserted a Kòberles trocar, which, of course, we should never do in dermoid cysts if we can help it. Immediately the trocar was stopped up by what I found later was a mass of hairs and sebaceous matter, so that I had some difficulty in keeping the abdominal cavity clean. However, she recovered without any greater trouble than a little abscess in the abdominal wall from one of the sutures.

Before demonstrating the specimen I wish to make a few remarks in regard to malignancy of dermoid cysts. As a rule, we regard a dermoid cyst as a benignant new formation, and a malignant character is here a rather rare exception. We make a distinction between malignancy of a dermoid cyst, *per se*, and malignancy from a combination of dermoid cysts with carcinoma or sarcoma. The malignancy of a dermoid cyst as such is very rarely seen. Kolaczek relates a case, operated upon by Martini, in which, besides a common dermoid cyst with a perfectly smooth surface, there was found in the walls of the peritoneal cavity small nodules in great number, the size of a millet seed and of a yellowish color. Many of these little tumors had a light-colored hair sticking out from their centres into the peritoneal cavity. Similar were seen in a case operated upon by Billroth, reported by Fraenkel.

Malignancy of a dermoid cyst from combination with carcinoma, sarcoma and myoma. These tumors originating in the tissues of the cyst are not so very seldom met with, and have been observed more commonly of late years, because a more minute microscopical examination is made now than in former years. Ohlshausen mentions as bearing upon this subject, a statement of Doran, that he had seen in several instances malignant tumors of the abdominal cavity follow extirpation of dermoid cysts. On examining the main wall of the specimen before us, we find on the dermoid island with its hairs and a plate of bone in the wall, the following unusual formations:

(a) A large black mole. It is of irregular lobulated shape, two by three inches in diameter, slightly elevated over the surrounding skin, and has a velvetty uneven surface without hairs. Microscopic examination shows the common structure of pigmented moles, which, as you will remember, has a great similarity to that of a sarcoma.

(b) A papilloma the size of a pea. You will see it outside of the mole on the skin over the bony mass. It is surrounded by a thick wrinkled skin beset with hairs. On transverse section it shows a solid centre covered with the pointed excrescences, resembling exactly a large wart with long papillæ, as we sometimes find them on the skin of the hand. A detailed microscopic examination and description of all the specimens is not as yet finished, but I intend to give it in a future discussion. It is sufficient, however, here to call attention to the important bearing, the two benignant new formations found in this cyst have upon the malignancy just spoken of. It is well-known that moles often furnish the soil for sarcomas, and that warts or papillomas for years benignant, sometimes all of a sudden commence to grow, because they are transformed into a carcinoma or sarcoma. The rapidity with which a dermoid cyst sometimes will grow involves a great nutritive hyperactivity. I can understand that this, in its turbulent way of forming tissues without an etiological object, could cause the physiological resistance to disappear, and thus open up the gates for malignant tumors.

Dr. A. Reeves Jackson read the following paper, entitled:

VAGINAL PRESSURE IN THE TREATMENT OF CHRONIC PELVIC DISEASE.

(See page 454).

Dr. Philip Adolphus: In the treatment of chronic pelvic disease by vaginal pressure, we may avail ourselves of the two methods of massage and columning the vagina. The latter has a much wider range in the treatment of pelvic disease than massage.

These methods have been hitherto applied to the removal of congestions, exudates, and recent slight adhesons of the serous tissues in the pelvis, which were in reach of vaginal pressure.

We often thought we had succeeded in removing by them old adhesions and bands, when merely recent effusions surrounding old deposits had become absorbed, just as nature will, without our aid, absorb a large pelvic effusion in a recent pelvic inflammation.

Neither method, however, can cause the removal of old cicatricial bands, firm adhesions and imbedded organs; and both are contra-indicated when inflammation of the serous tissues exist.

But where dilatation and congestion are present, and comparatively recent adventitious and hyperplastic tissues are to be removed, the stimulant and alterative influence of pressure on the pelvic vessels induces absorption by either of these methods.

The treatment by massage will not, in future, be resorted to as often as formerly; for it is inefficient in its methods, dangerous in its tendency, as well as troublesome and indelicate to the physician and patient.

The sole object of massage is to induce sufficient irritation in order to effect absorption. But the tamponnement of the vagina does much more than massage.

It supports and relaxes tense ligaments; elevates the movable or adherent vagina, bladder, uterus and ovaries, provided they are not adherent to the pelvic walls; depletes congested, inflamed and subinvoluted organs; overcomes spasm and irritation, and induces physiological rest in the parts.

Tamponnement per vaginam is therefore indicated in all cases where pelvic tenderness is present which is not due to an acute attack, or where absorption is needed; it is efficient in cases of mal-position and

prolapse of the uterus, ovaries, ligaments and vagina, where elevation of the organs and mechanical support are required, preparatory to the use of pessaries, or where these cannot be borne.

Columning the vagina is effected in the knee-elbow position by means of Sim's or Simon's speculum. A large pinch of iodoform, boric acid or salicylic acid is first applied to the cervix, a few tampons saturated with glycerine are laid in the vault of the vagina, and then ordinary cotton wool, absorbent wool or iodoform gauze is systematically packed into the vagina, to remain there for three or four days. This packing is to be renewed until the effects are produced which the practitioner desires. The patient is not obliged to remain in bed, and the pelvic, sacral and hypogastric pains, together with urethral irritability, are frequently relieved in a short time by this method, which is altogether a more successful, cleanly and decent mode of procedure than that of massage.

DR. JAMES H. ETHERIDGE: I have nothing additional to say except that the continued use of this method in many selected cases has produced most desirable results. But I would protest most emphatically against being understood as recommending it for every trouble of a pelvic nature. For the class of cases Dr. Jackson has enumerated I think it a vastly superior treatment. I was much impressed with the article of Dr. Taliaferro. He tampons the uterus cavity, with the patient in the genu-pectoral position, using a speculum of his own device, which is flanged at the lower end so as to separate the posterior portion of the vaginal orifice, the cervix being held steadily down by the vulsellum, and with a long-toothed forceps he pushes the cotton into the uterine cavity. The true explanation of the benefit which comes from this treatment is, that by elevating the uterus the pressure from its great weight is relieved. There is a mechanical obstruction to the return of blood from the uterus, and what is done by the tampon is to push up the uterus and permit its decongestion, and along with that comes the improved nutrition of the organ, and the reflex symptoms in the way of pain, menstrual disturbances and the like readily disappear. I cannot tell exactly how I was put upon this method of treatment; I don't claim that it is anything new. Several years ago Dr. Bozemann, of New York, tamponned the vagina, calling it "columning the vagina. He spoke of several cases of positive elongation of the posterior wall of the vagina with this continual pressure. His paper was published in full in the transactions of the American Gynæcological Society.

DR. H. T. BYFORD: I agree with Dr. Etheridge, that any pressure that can be made by the vaginal pack could not cause the relief. The benefit of pressure upon enlarged veins in any part of the body, as in the leg or testicle, is only temporary unless some other curative influence be added. Nor do I think that a low position of the uterus causes the venous stasis, for this is not found in all cases, and often is found when the uterus is held high up by contracted and indurated sacro uterine ligaments. The veins are large, long and tortuous, and are made to admit of considerable change in position of the uterus in almost any direction. The rapid improvement comes from the support to the uterus, and sometimes also to the ovaries, taking away the traction upon inflamed and indurated ligaments, and thus promoting the absorption of exudations that either diminish the caliber of the veins or prevent them from accommodating themselves to the position of the uterus and its adnexa. This relief of strain and promotion of absorption in the pelvic tissues is the great remedy for subinvolution in the subacute stage, the same as rest in bed is the remedy in its acute stage, viz., soon after labor. It is in the subacute stage of pelvic disease that the vaginal pack finds its great sphere of usefulness. Dr. Jackson's second case serves as a good illustration. When the inflammation and induration are in the sacro uterine ligaments, two or three soft glycerine tampons, made of the best ordinary cotton batting (not the absorbent), placed under and in front of the cervix every second day at the office, and left till the next night, will often relieve the traction and bring about rapid improvement. When it becomes necessary to apply the complete pack, we will get the best effects by so placing the cotton and cotton wool as to relieve the traction upon tender parts, which should be found beforehand by a careful diagnosis.

DR. JACKSON: I think the subject has been very thoroughly discussed and the principal of the treatment clearly illustrated. The important fact is, that it is not a difficult method of treatment; that it is efficacious there can be no doubt; the clinical facts justify this assertion. The method pursued by Dr. Bozemann seems to me to be peculiarly objectionable. Strapping a woman on to a machine for the purpose of packing the vagina, seems both irksome and unnecessary. I am very glad there is such unanimity of opinion in regard to the clinical efficacy of this method of treatment.

OBSTETRICAL SOCIETY OF PHILADELPHIA.

Stated Meeting, March 3, 1887.

THE PRESIDENT, THOMAS M. DRYSDALE, M.D., IN THE CHAIR.

(*Concluded from page 443.*)

DR. HOWARD A. PARDEE read a paper entitled

PERINEORRHAPHY AS PERFORMED BY DR. W. GILL WYLIE.

(See page 432, JOURNAL of April 16.)

DR. PARISH had for years performed a similar operation, but carried his denudation farther up the vagina, and thus avoided the formation of a pocket-like place in which discharges accumulate. He has found it in some cases a cure for constipation.

DR. BALDY did not think Dr. Pardee understood Emmet's operation.

DR. PRICE prefers Emmet's operation.

DR. H. A. KELLY thought this an unsatisfactory modification.

DR. PARDEE was well acquainted with Emmet's

operation as performed two and a half years ago. The obliteration of the rectocele in that operation was by drawing down a stretched wall, leaving it thin over the rectocele. In his experience opium had not been needed after the operation to control pain. The external stitches are needed to secure a neat external healing.

Dr. Joseph Price exhibited a specimen of

DOUBLE PYOSALPINX, WITH COEXISTING OVARIAN CYSTOMA ON BOTH SIDES.

Mrs. H., æt. 30 years, two children, labors normal, complains of irregular and profuse bleeding; constant pelvic pain, greatest on left side. General health poor. Examination revealed a small fibroid on posterior wall of the uterus; a cyst on the right side extending high up, inflammatory masses laterally. *Operation*, section. The appendages on both sides were distended by pus and generally adherent, left ovary cystic and suppurating, and matted to the pavilion of tube and the sigmoid flexure. The right ovary was cystic and adherent to the vermiform appendix. The pelvis was filled with an irregular conglomeration of cysts and pus-tubes, intimately adherent to pelvic organs, and overlying all this were the bladder and omentum, also firmly adherent. The omentum was perforated at two points, and the mass was removed by careful dissection. A glass drainage-tube was used. Recovery was rapid and perfect; no opium, no catheter, tube out on fourth day, stitches out on seventh day.

Dr. W. H. Parish reported a

PORRO-MÜLLER OPERATION,

performed by Drs. O. H. Allis and W. H. Parish because of an impacted shoulder presentation. Mary Leparo, æt. 26 years, of small stature but good general health. When about six months pregnant she had been hooked by a cow and narrowly escaped a miscarriage. She reached full term and had been in labor seven days when Dr. Allis was telegraphed for by Dr. Groom, of Bristol, Pa., in whose care the patient had been. The membranes ruptured four hours after the pains began. On the sixth day the pains became more severe and the cord prolapsed. About the same time a hand protruded from the vagina. The child died about this time. Version had been tried by Dr. Groom and others, without and with anæsthesia, but could not be effected. The child was evidently of large size; the patient was restless, exhausted, and with a pulse of 120 per minute. The pains were constant and the uterus was firmly contracted above the child. The patient's surroundings were unfavorable, the ignorance and poverty of the family rendered proper nursing impracticable. An offensive fluid was escaping from the uterus; the cervix was rigid and but slightly dilated. It seemed impossible to deliver the patient by either cephalic or podalic version, and the conclusion was made that evisceration was not advisable, because there was every reason to believe that the uterus had done itself irreparable harm by reason of its prolonged retraction against the eminences of the child. The contusion incident to the retraction and the admission of atmospheric air over a period of seven days, with the character of the discharge, rendered it certain that gangrenous changes had been going on in the endometrium and placenta, and that septic inflammatory action of the uterine lymphatics had already begun. The patient was in a condition of exhaustion. Evisceration alone would not have sufficed to secure delivery; it would have been necessary to have bisected the child in the lumbar region and to have removed separately the lower and upper sections. The condition of the uterus and the small size of the pelvis would have rendered this procedure a lengthy and an exceedingly difficult one, and would have added additional serious injury to the uterine tissues. The removal of the child would have left a putrefying endometrium and septic lymphatics. I felt certain the patient would die if thus delivered. All present thought Cæsarean section indicated. The operation was performed by Dr. Allis. The uterus was elevated from the abdomen and a rubber tubing was placed about the cervix, securely controlling the hæmorrhage. Escape of fluids from the uterus was guarded against by means of warm antiseptic pads adjusted about and over the abdominal incision. A vertical incision was now made, and the child extracted without difficulty. The placenta was not in the line of incision and there was no hæmorrhage. Two transverse pins were carried through the cervix, and a constricting wire was substituted for the rubber tubing about the junction of the cervix with the body of the uterus. With the uterine body were removed also both broad ligaments and their contents. The uterine stump was secured externally by means of the transfixion pins, and wounds closed by carbolized silk sutures. Antiseptic measures were taken throughout the operation, but the spray was not used. Immediately after the operation the pulse was 130 per minute. The patient died in forty-eight hours, apparently from heart failure. A full account of this case will be found in the *American Journal of Obstetrics*.

Dr. R. P. Harris remarked that the Cæsarean operation has been performed in the United States in cases of impaction of the fœtus in a transverse position twelve times. In eleven cases the fœtus was dead. In the twelfth it was saved, but the operation was believed to have been uncalled for by a physician who performed laparotomy with success after the woman had ruptured her uterus, in her next labor. The pelvis had been computed to have a conjugate and a transverse diameter of three inches. In one other of the twelve cases the pelvis was deformed, and the woman saved. The arm was protruding in seven cases, five of which recovered. In two the shoulder presented; one saved; and in three the presenting part is not named. Of the twelve cases nine, or 75 per cent., recovered. Of the three cases lost one was ninety-six hours in labor, three days in charge of a midwife. Another wes twenty-six hours under a midwife who had given ergot and ruptured the membranes; and the third had been long in labor, but time not stated. Her death was caused by fright and exhaustion; she rose from her bed to defend her mother against her husband, who had come home drunk on the third day. He knew

of but one case of impaction in Europe in which the Porro operation was performed, but in this the pelvis was deformed. The operation was by Fehling, of Stuttgart, and the woman was saved. Putridity of the fœtus may require the removal of the uterus in a case of impaction. One woman was saved, without its removal, in the United States after seven, and another after ten days of labor, by uterine suturing to prevent escape of fluid; but it is safer to remove the organ where there is danger of poisoning, and particularly where the patient appears to be already threatened with it. In the Porro case of Candido Ramello, of Turin, the woman had been six days in labor, the fœtus was putrid, and the mother in great danger of septicæmia. The removal of her uterus with its contents was followed by an improvement of her symptoms, and she recovered.

FOREIGN CORRESPONDENCE

SURGERY IN LIVERPOOL AND MANCHESTER.[1]

I spent two days in Liverpool, and became personally acquainted with Rushton Parker, Reginald Harrison and Mitchell Banks. Parker lectured on "Sarcoma" to a class of fourteen students. Looking at his class-list I noticed twenty-seven names. The lecture was delivered in a colloquial style and gave evidence of a good clinical acquaintance with his subject, but no deep points in histology or pathology were elaborated. I visited with him the Royal Infirmary and the various departments of the University, which is located in close proximity to the hospital. The chemical branch of this institution was erected and endowed by some of the rich manufacturers of the city, and is a model of usefulness and perfection.

Harrison is a man of middle age and is an enthusiast in his specialty. He showed me several cases of rupture of the urethra followed by extravasation where he had made external urethrotomy and incisions through the infiltrated tissues. He places great stress upon the necessity of removing the extravasation by firm compression, relieving the tissues at once of the fluid in the same manner as squeezing a sponge. The result of this procedure is to prevent sloughing and hastening the suppurative process. He drains the bladder only for a few days with his drain. He is a firm advocate of boutonnière for diagnostic and therapeutic purposes. He looks upon this operation as a trivial procedure almost devoid of danger. He introduces the drain along the groove of a Wheelhouse's gorget. He also demonstrated upon several cases the advantages of his "whip bougie." This instrument is about twenty inches in length, and if previously warmed is so flexible that it doubles upon itself, when fully introduced, without doing damage to the walls of the bladder. As the instrument is conical in shape, one introduction is followed by the same degree of dilatation as the introduction of a number of the ordinary filiform bougies. He favors treatment of strictures of urethra by gradual dilatation, and reserves the cutting operation for exceptional cases.

He is not favorably inclined to the crushing operation for stone in the bladder, and showed a case of lateral operation done a few days before where the wound had almost completely healed.

In plastic operations upon the urethra he performs the buttonhole operation, and drains the bladder as a preliminary measure. You can readily conceive the advantages accruing from this precaution, as it secures to the wound the two essential conditions for rapid healing: rest and the possibility of an aseptic healing. In draining the bladder he stitches the margins of the wound closely around the tube so as to prevent leakage between the drain and the soft parts.

In hypospadias and epispadias he makes the new urethra by tunneling the tissues from the apex of the glans penis to the abnormal opening, and secures patency for the new channel by introducing a drainage-tube until the healing process is completed. As a last step the abnormal opening is accurately closed over a catheter and the urethral continuity is restored. To guard against subsequent constriction of the new canal the patient is instructed to introduce a good-sized bougie at least once a week.

Mr. Banks operated upon a case of carcinoma of the breast, and although the axillary glands did not appear to be involved, a clean dissection of the axilla was made to the axillary vessels. Wherever a vessel was recognized it was ligated at two points and divided between the ligatures, consequently very little blood was lost during the operation. A spectator was readily convinced that the operator had been for years a teacher of anatomy. Spray was used, as I was told, more for the purpose of irrigating the wound than for its supposed effect in sterilizing the atmosphere. The wound was completely closed with silver sutures and drainage established in the lower posterior recesses of the wound. As an antiseptic dressing sublimated wood wool cushions are are used. I received the impression that dressings are too frequently changed in this hospital, thus depriving the wounds of that perfect rest required in a typical wound-healing and exposing the patients to the risks of subsequent infection during the frequent dressings.

Mr. Banks also operated upon a lad who had received a scalp wound nearly three months ago. No brain symptoms were present after the injury or for some time subsequently. During the last few weeks rise in temperature, slight suppuration of wound. For the last few days greater rise in temperature, vomiting and sopor; both pupils contracted. At site of injury over the left frontal region the wound presented a granulating appearance and a probe could be passed through a small aperture into the skull. Probable diagnosis, suppurative pachymeningitis. The field of operation was carefully disinfected, and while patient was under the influence of chloroform the opening in the bone was enlarged with bone forceps. The dura bulged into the wound and did not

[1] This letter from Dr. N. Senn is published by the kind permission of Dr. Christian Fenger and Dr. Senn. Others will appear.

pulsate. The meninges were incised with a sharp bistoury without having made an exploratory puncture, but no pus or any other fluid remained, and after some superficial probing the wound was dressed without making any further attempt to search for a deep-seated abscess.

Mr. Thos. Jones, of Manchester, one of the surgeons of the Royal Infirmary, is lecturer on Clinical Surgery in Owen's College, and a surgeon who is esteemed not only in his own city but throughout the United Kingdom. The Infirmary contains about 300 beds, and, although an old building, is well ventilated and well furnished. Typical resections of the hip and knee are in favor, and many cases were shown in which this operation had been performed. Antiseptic wound treatment is followed, but many of the more minute details are not carried out. Sublimated wood wool dressings are used almost exclusively. Many of the operation wounds showed evidences of imperfect wound treatment. For immobilization of the lower extremity Thomas's splint is used in preference to any other. Fractures are treated with removable splints until swelling has subsided before the plaster-of-Paris splint is applied.

Mr. Hardy was met in the operating-room, where he was putting on the finishing touches in a rhinoplastic operation made by transplanting distal phalanx of left index finger for a bony support and a covering of a flap taken from the anterior surface of the left arm. Interval between the several steps of the operation of about three or four weeks. Present cosmetic results quite satisfactory. He has done this operation three times, and always with a satisfactory result.

I was shown a case of myxœdema in a girl 21 years of age. Mr. Jones also called my attention to a case of metastatic carcinoma following scirrhus of the breast, of more than twenty years' standing, which had resulted in a pathological fracture of the left femur and right humerus. The interesting feature in this case consisted of rapid swelling at the point of fracture with subsequent subsidence of swelling, but no union between the disunited bones.

A case of multiple fibromata of a congenital origin also attracted my attention. The patient was a male about 50 years of age, thousands of tumors covering the entire surface of the body from the scalp to the plantar surfaces of the feet.

In the afternoon I visited Owen's College in company with Dr. Sinclair, who introduced me to the eminent physiologist, Dr. Stirling, the translator and annotator of Landois' "Physiology."

A visit to a private hospital for women and children with Dr. Sinclair, the attending gynecologist, proved interesting and profitable. I was shown three cases in which abdominal section had been done for different indications; in all of them the dressing was removed and the glass drains emptied by suction with a syringe mounted with a rubber tube. In all of them a small quantity of a reddish serum was removed and the drains washed out with ¾ per cent. solution of chloride of sodium. The temperature was normal, or nearly so, in all of them. The operations are all done under the strictest antiseptic precautions. Dr. Sinclair is a gynecologist who has the interests of his profession at heart, and who has a promising future before him.

I visited Dr. Ross at his home with Dr. Robertson. He lives in a comfortable, almost luxurious mansion in one of the most beautiful suburbs of the city. With a capacious park surrounding his house, completely isolating him from the turmoils of a great city, we can readily conceive how a man of his intelligence and indomitable energy should accomplish so much. I met him on the following day in his wards at the Infirmary, and was pleased to follow him in his elaborate description of three cases of peripheral neuritis then under his care. The diagnostic tests were applied in presence of the class and could not fail to bring conviction to all of those who were fortunate enough to be within his hearing.

I also went to Oldham, the home and field of labor of Dr. Robertson, who showed me his cases at the Infirmary, a neat hospital containing 90 beds. Everything in the wards showed perfect discipline and the most careful management. Dr. Robertson is a young physician who furnishes a good illustration of what can be accomplished by modern aggressive surgery. Three simple cases of abdominal section had recently been done and had proved fatal within twenty-four hours from acute septic peritonitis. A search for the cause showed that defective drainage had occurred by the leakage of sewer pipes in the basement. The last case was operated upon in a cottage near the hospital, and was progressing favorably. At this place I had an opportunity of examining a patient with a fusiform aneurism, where the pulsations were distinct, but no bruit could be heard on auscultation. The patient contracted syphilis many years ago, and this disease was undoubtedly the result of mes-arteritis.

In the Manchester Royal Infirmary I saw Mr. Whitehead make a cholecystotomy. The patient was a male, about 50 years of age, who for four months had suffered from periodical pain in the region of the gall-bladder, followed by progressive jaundice. On palpation it was thought that the margins of the distended viscus could be felt. Under spray an incision was made parallel to and about a finger's breadth below the costal arch on the right side, sufficiently long for the operator to introduce two fingers for making the necessary exploration. As this opening appeared to be inadequate, another incision was made, at a right angle with the first, in a downward direction, when the gall-bladder, distended to the size of a small pear, was readily brought into the wound for inspection and palpation. A superficial examination showed that it contained no stones, but as its contents could not be emptied by compression, and palpation of the bile ducts did not satisfy the operator as to the presence or precise location of a probable stone in the ducts, the organ was stitched into the wound in the usual manner, to be opened and explored after adhesions had been secured.

I visited the Pendlebury Hospital for Sick Children, about four miles from the centre of the city, with Mr. Wright, one of the attending surgeons. This institution has a capacity of 300 beds, is con-

structed upon the pavilion plan and located in one of the most salubrious suburbs of the city. It is a model of architectural perfection. Mr. Wright is a most careful and prudent surgeon, and has done most excellent work. He operates early in cases of hip joint disease, but has abandoned typical resections of knee joint disease in favor of arthrectomy and atypical resections whenever such a course appears practical. He showed six cases of resection of the hip joint done recently, and most of them were without temperature. Fixation of limb is secured by means of Thomas' splint. The spray is used during the operation, and sublimated wool dressings are in general use. For fixation of dressing the elastic webbing bandage is frequently used. Tubercular glands are removed early, and the results of this method of operation have been satisfactory.

Mr. Wright strongly advocates radical operation for hernia in children, and in his hands the operation has been followed by only one death out of a great number subjected to this treatment. He does not remove the sac, but secures perfect coaptation by making with a needle, armed with a catgut ligature, numerous points of transfixion, and tying the ligature.

The nurses are well trained and render intelligent assistance at operations, as was demonstrated by a number of operations made during my presence. In my next letter I will report upon the result of my observations in Glasgow, Aberdeen and Edinburgh.

N. SENN.

NECROLOGY.

DR. JAMES STEWART JEWELL.

By the death of Dr. Jewell the American medical profession has lost a member of whom it has had reason to be proud in all respects. A self-made man in every sense of the word, who had obtained his thorough education solely through his own unaided efforts, he had risen to a position of great eminence as authority in the line of nervous and mental diseases, and as a recognized leader in medical thought. With his extensive general and medical knowledge he combined a rare good judgment and tact, as well as a dignified and kind manner which made him the ideal representative of the medical profession in the eyes of the public. His thorough culture, coupled with modest though firm self-reliance, could not fail to impress his patients, as well as the public in general, with the belief that his opinion on any medical subject reflected, indeed, the best and most recent views that medical science of the day could suggest.

Dr. Jewell was born in 1838, on a farm near Galena, Ill., where he remained until his 14th year. During this time his school life did not comprise over one year, and whatever knowledge he did acquire was picked up during times which others would have considered but proper rest from labor, unusually severe for one of his age. Subsequently his family removed to Marion, Ill. There he continued his farm work, by which he contributed largely to the support of his family. His thirst for education, aided by an unswerving persistence of purpose urged him to procure books whenever possible, often traveling long distances on foot to obtain the loan of some coveted volume. Over these treasures he would pore—even all night—and yet not allow them to interfere with the necessary daily routine of farm labor. His taste for science, and especially medicine was encouraged by Dr. S. W. Mitchell, of Corinth, whose library in later years afforded him the means of improving his education. It was to him as well that he owed his final preparation for college life. He entered Rush Medical College in Chicago in 1859, and finished in the following year in the first class of the newly founded Chicago Medical College. After passing a term as Interne in Mercy Hospital he returned to a country practice in Williamson County, but finally entered the army and served as surgeon in an Illinois regiment during the last year and a half of the war. In 1864 he returned to Chicago to fill the position of assistant to the chair of anatomy in the Chicago Medical College, but by reason of the resignation of the previous incumbent he was at once promoted to the full professorship. Having married he now devoted himself to general practice in this city until 1868, when he left for an extended tour through Europe and the Orient. Upon his return in 1870, the Chicago Medical College created especially for him the chair of nervous and mental diseases, to which specialty he then devoted himself exclusively. Without publishing much he became so favorably known in the profession through his teaching and personal intercourse that when he began his *Journal of Nervous and Mental Disease* in 1874, it was at once a complete success. This magazine was the first organ devoted to that specialty in the English language. Any one who has followed the American periodical literature since that time, can but admit that Dr. Jewell's journal has exerted a stimulating influence on the growth of that specialty in this country, as well as on the general diffusion of a knowledge of scientific medicine. In 1875, Dr. Jewell contributed largely to the foundation of the American Neurological Society, of which he subsequently became president. As the natural result of his well directed efforts he acquired a very large consultation practice in his specialty. In the field he had chosen he encountered naturally a great many sufferers from chronic diseases of unusual obscurity, and in this very line of practice his remarkable thoroughness and penetrating judgment enabled him to achieve practical triumphs which will keep his memory alive for years to come throughout the entire northwest.

Some six years ago his health began to fail. With many regrets he deemed it best to relieve himself of the severe tax which the management of his journal put on his time. In spite of continued suffering and distress he continued his arduous practice and managed for a time to struggle vigorously against his physical misfortune, increased by the blow which he received on the death of his wife. Repeatedly he broke down, however, and was forced to pass part of his winters in the south. About one year ago he began the *Neurological Review*, but his health did not

permit him to continue its publication for more than a few numbers. Although sinking steadily during the present winter, he did not abandon his practice until within a few days of his death on the 18th of April. He leaves behind a family of four, two sons and two daughters.

While Dr. Jewell's name was not coupled with any important discoveries in medicine, his teaching, both by word in the college and by means of his many valuable contributions to periodical literature, has been of unmistakable benefit to the profession. He was always an enthusiast in the application of all scientific data for scientific purposes. Thoroughly acquainted with everything that was being done in the scientific world, his steady aim was to investigate disease in the light of the most recent physiological developments. For many years he has been busy on a work on the pathology of the nervous system, on the basis of many hundreds of clinical records of unusual thoroughness, but whether enough of the manuscript has been completed to publish the book, is as yet uncertain. He had formed plans and collected material besides for several monographs, but it is unfortunately not yet positive whether his literary legacy is realizable.

Personally Dr. Jewell possessed a rare amiability and kind disposition not likely to be forgotten by his many friends. No man could have achieved his position under such difficulties, and surmounted such obstacles as he did during youth without possessing an undaunted energy and indomitable persistence of purpose. His undisguised contempt for all actions of a small calibre could not but arouse the opposition of some few envious of his success; but withal he had no open enemies. May the memory of a truly grand life serve as a model for others.

H. G.

INTERNATIONAL CONGRESS.

Section in Psychological Medicine and Nervous Diseases.

President, JUDSON B. ANDREWS, M.D., Buffalo, N. Y.

The following titles of papers have been presented for this Section: "The True Nature and Definition of Insanity;" "Lunacy in Scotland;" "Nervous Degeneration and Syphilis" (selected as a subject for Discussion); "Locomotor Ataxia and Syphilis;" "Original Investigations in Cerebral Syphilis, with Microscopic Preparations, etc.;" "Relation of Syphilis to General Paralysis;" "State Policy in Relation to the Construction of Buildings for the Insane;" "Hospital Arrangements and Classification as relates to the care and comfort of Patients;" "Modern Hospital and Asylum Structures for the Insane;" "The Borderland and Early Symptoms of Insanity;" "The Religious Delusions of the Insane;" "Mental Manifestations of Insanity caused by Sunstroke;" "The Remissions, Intermissions, and Manifestations of Insanity in an Individual;" "General Paresis; Its Varieties and Differential Diagnosis;" "Legal Relations of Epilepsy;" "Obscure Forms of Epilepsy;" "Sleep;" "Miliary Aneurismal Disease of the Spinal Cord and Brain Axis;" "Deformity of the Brain in its Relation to Congenital and Inherited Mental Disease;" "Cases Illustrating the Prow-shaped Cranium with Neurotic Disease;" "Remarks on the Etiology and Pathology of Thomsen's Disease; "Tendon Reflex in Hemiplegia;" "Hyperæsthesia;" "Castration as a Remedy in Nervous Affections;" "Occupation in the Treatment of Insanity;" "The Treatment of Neuralgia from the point of View of the General Practitioner;" "The Disease of Inebriety and its Treatment;" "The Physiological and Therapeutical Effects of Currents of High Tension (Franklinization);" "The Private Treatment of the Insane, as Single Patients in England;" "Mental Diseases Produced by Changes in the Vessels at the Base of the Brain;" "An International Classification of Insanity;" "Monomania;" "Cocaine Inebriety;" "General Paralysis in Dogs." Several titles are yet to be received.

Among the foreign physicians who contribute to the foregoing list of titles of papers to be read, or who intend to be present and participate in the scientific work of the Section, are Sir James Creighton Brown, Drs. Rudolf Arndt, Buiswanger, Geo. Fielding Blandford, Bouchereau, Francis Pritchard Davis, Maurey Deas, J. Langdon Down, Edward East, A. Eulenberg, E. T. Girtstrom, V. Hinze, Megalhães Lemos, H. Rooke Ley, E. Mendel, Wm. Julius Mickle, Arthur Mitchell, O. Muller, M. Rosenthal, George H. Savage, Seymour J. Sharkey, J. Beverly Spence, Valdemar Sternberg, D. Hack Tuke, John Batty Tuke.

Communications relating to papers, discussions, etc., should be addressed to the Secretary of the Section, Dr. E. D. Ferguson, Troy, N. Y.

Section of Laryngology.

PRESIDENT.

W. H. Daly, M.D., 71 Sixth Ave., Pittsburg, Pa., U. S. A.

SECRETARIES.

Wm. Porter, M.D., 3137 Lucas St., St. Louis, Mo., U. S. A.

D. N. Rankin, A.M., M.D., 85 Lincoln Ave, Allegheny, Pa., U. S. A.

Dr. Ottakar Chiari, 14 Elisabeth Strasse, Vienna, Austria.

Dr. E. G. Moure, 2 Cours de Tournon, Bordeaux, France.

The following gentlemen have signified their intention of presenting papers:

Dr. B. Baginsky, Berlin, Germany; Dr. S. N. Benham, Pittsburgh, Pa.; Dr. A. Cartaz, Paris, France; Dr. W. E. Casselberry, Chicago, Ill.; Dr. W. Cheatham, Louisville, Ky.; Dr. A. J. Coey, Chicago, Ill.; Dr. W. W. Cole, Allegheny, Pa.; Dr. W. F. Coomes, Louisville, Ky.; Dr. Ephraim Cutter, New York, N. Y.; Dr. H. H. Curtis, New York, N. Y.; Dr. W. H. Daly, Pittsburgh, Pa.; Dr. T. D. Davis, Pittsburgh, Pa.; Dr. C. M. Desvernine, Havana, Cuba; Dr. Richard Ellis, Newcastle-on-Tyne, England; Dr. Richardson Gray, Orange, N. J.; Prof. Joseph Gruber, Vienna, Austria; Prof. Hack, Frieburg in Baden, Germany; Dr. J. H. Hartman, Baltimore,

Md.; Dr. Herman E. Hayd, Buffalo, N. Y.; Dr. D. A. Hengst, Pittsburgh, Pa.; Dr. Theodore Hering, Warsaw, Poland; Dr. Camalt Jones, London, England; Dr. H. Jones, London, England; J. P. Klingensmith, Blairsville, Pa.; Dr. Paul Koch, Luxembourg, France; Dr. H. Krause, Berlin, Germany; Dr. Geo. Mackern, Buenos Ayres, Argentine Republic; Dr. Geo. W. Major, Montreal, Canada; Dr. F. A. Mandeville, Rochester, N. Y.; Dr. F. Moura, Paris, France; Dr. D. F. Massei, Naples, Italy; Dr. A. W. Orwin, London, England; Dr. M. C. O'Toole, San Francisco, Cal.; Dr. Wm. Porter, St. Louis, Mo.; Dr. D. N. Rankin, Allegheny, Pa.; Dr. J. M. Ridge, Camden, N. J.; Dr. J. O. Roe, Rochester, N. Y.; Dr. John A. Robison, Chicago, Ill.; Dr. O. Rosenbach, Breslau, Germany; Dr. A. Schnee, Nice, France; Prof. John Schnitzler, Vienna, Austria; Dr. Schaumacher, Achen, Germany; Dr. Carl Seiler, Philadelphia, Pa.; Dr. J. G. Sinclair, Nashville, Tenn.; Dr. F. Semeleder, City of Mexico; Dr. Max Stern, Philadelphia, Pa.; Dr. J. A. Stucky, Lexington, Ky.; Dr. Richard Thomas, Baltimore, Md.; Dr. W. McNeil Whistler, London, England; Dr. Edward Woakes, London, England; Dr. Zeim, Danzig, Germany.

ASSOCIATION ITEMS.

OPHTHALMOLOGICAL SECTION.

The Thirty-eighth Annual Session will be held in Chicago, Ill., on Tuesday, Wednesday, Thursday and Friday, June 7, 8, 9 and 10, commencing on Tuesday, at 11 A. M.

"Sympathetic Ophthalmia." Dr. C. M. Hobby, Iowa City, Iowa.

"Some Observations on Displacement of the Crystaline Lens, from Congenital and other Causes." Dr. J. L. Thompson, Indianapolis, Ind.

"Hydrobromate of Hyoscine as a Mydriatic." J. M. Ray, M.D., Louisville, Ky.

"Treatment of Hypopyon Keratitis with frequent irrigations of Sublimate." Dr. F. C. Hotz, Chicago, Ill.

"Some of the Ophthalmological Clinics of Europe." J. W. Heustis, M.D., Pittsburg, Pa.

HOTEL RATES.

The following hotels in Chicago have thus far commuted their rates to delegates and members and their families who will attend the annual meeting in June—all promising first-class accommodations:

Grand Pacific Hotel.—A discount of 50 cents to each person per day in all rooms, except those at $3.00 per day, on which there will be no discount. By this arrangement a $3.50 room with bath may be had for $3.00, a $4.00 room for $3.50 for each person, and so on. A club room will also be placed *free* at the disposal of the Association for headquarters, if desired.

The Sherman House will give 50 cents *per capita* off regular rates, which are $3.50 to $5.00 *per diem*, as well 50 cents *per capita* off $3.00 per day rooms where more than one person occupy a room. Such committee rooms, etc., as may be required will be placed at the disposal of the Association free of cost. No cots are ever used at this hotel, and every one will be given a good bed.

The Tremont House offers 50 cents discount from regular rates, which are from $3.00 to $4.50 *per diem*. This rate applies to each person or delegate, both included.

The Palmer House gives a rebate of 50 cents *per capita* on the American plan, when two occupy a room. On $1.00 rooms, (European plan) a rebate of 25 cents *per capita* where two occupy a room. On rooms for which the rate is $1.50 and upwards, a rebate (on the European plan) of 50 cents *per capita* is offered.

The Briggs House.—The rates will be $2.00 to $2.50 per day for their *best* rooms, meals included, a reduction of 50 cents per day on transient rates.

The above hotels are located within a few blocks of Central Music Hall, the place where the general (forenoon) sessions will be held. Additional information as to hotel rates will be published in subsequent issues of THE JOURNAL.

RAILROAD RATES for delegates will be published in full next week. They will be one and one-third (1⅓) fare for the round trip issued on the certificate plan, as follows:

Tickets for return journey will be furnished those having paid full fare to place of meeting who procure tickets not more than three days before the Association assembles, if presented within three days after the date of adjournment. Some forty different railroads of the country having already agreed to accept return tickets issued under this plan. The names of these railroads will also be published in next weeks' issue of THE JOURNAL.

CHARLES GILMAN SMITH, M.D.,
Chairman Local C mmittee of Arrangements.
LISTON H. MONTGOMERY, M.D.,
Chairman Committee on Transportation.

MISCELLANEOUS.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT, U. S. ARMY, FROM APRIL 16, 1887, TO APRIL 22, 1887

Capt. Edward B. Moseley, Asst. Surgeon, ordered for duty at Whipple Bks., Ariz.. S. O. 89, A. G. O., April 18, 1887.

APPOINTMENTS.

Charles E. Woodruff, Julian, M. Cabell, to be Asst. Surgeons with the rank of First Lieut., to date from April 14, 1887.

OFFICIAL LIST OF CHANGES IN THE MEDICAL CORPS OF THE U. S. NAVY, DURING THE WEEK ENDING APRIL 23, 1887.

Russell, A. C. H., P. A. Surgeon, U. S. N. ordered to duty at Naval Laboratory, New York. May 2, 1887.

Heffenger, A. C., P. A. Surgeon, U. S. N., ordered to Widow's Island, Me., to superintend building a naval hospital, wharf and other improvements, under instructions of the Surgeon-General of the Navy.

Woodruff, Chas. E., Asst. Surgeon, U. S. N., resignation accepted, to take effect April 8, 1887.